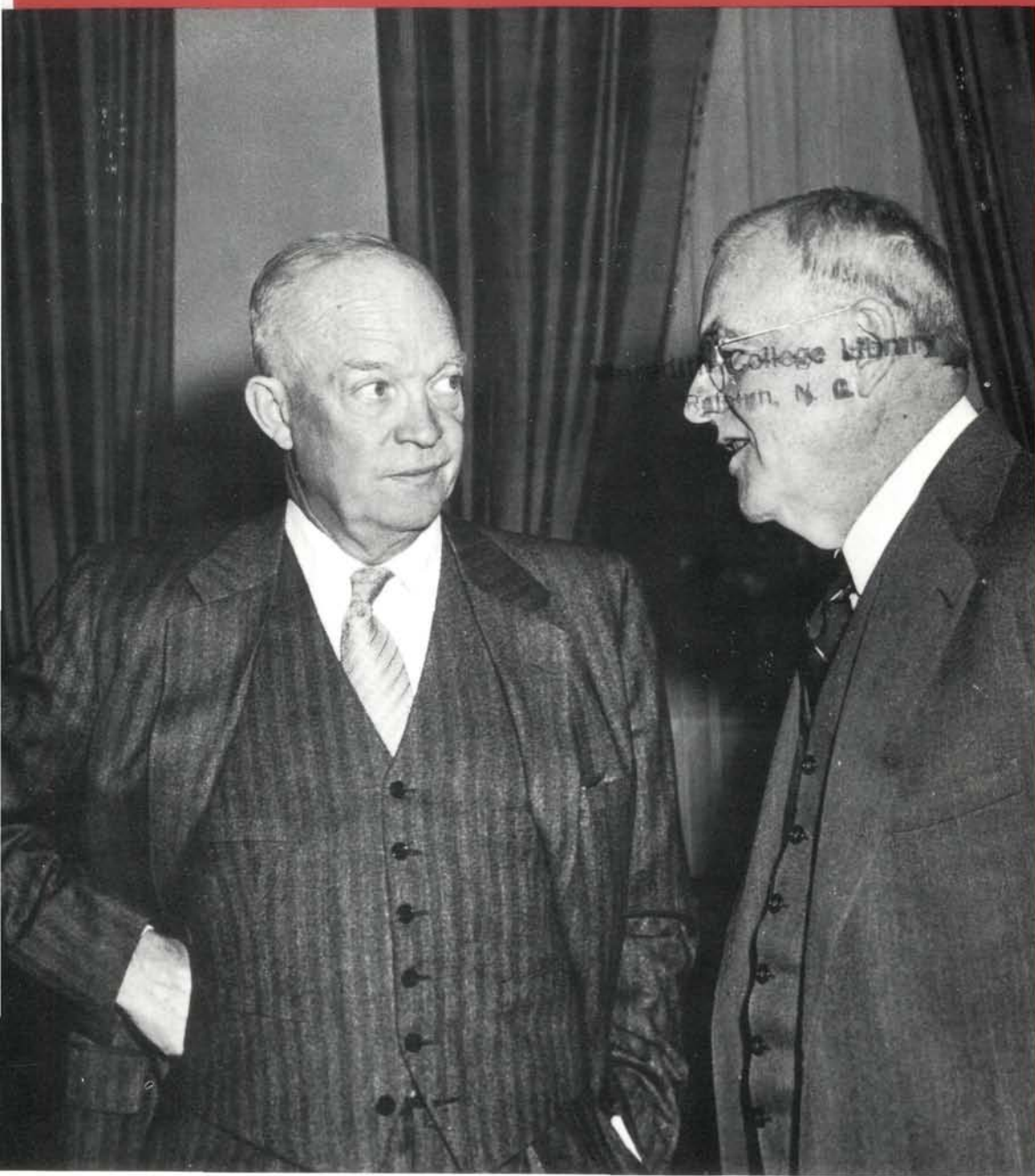


VOLUME 94 • NUMBER 4 • OCTOBER 1989

The American Historical Review

AMERICAN HISTORICAL ASSOCIATION



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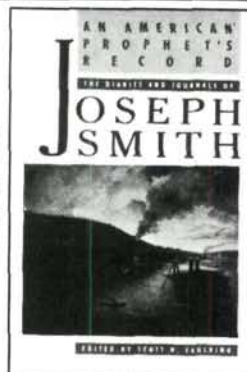
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Poll is former administrative vice-president of Western Illinois University.

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Cover illustration: President Dwight D. Eisenhower and Secretary of State John Foster Dulles at the beginning of 1954, about the time the Eisenhower administration unveiled its "New Look" in defense planning and announced the policy of "massive retaliation." Photo courtesy of the National Park Service. See the article in this issue by H. W. Brands, "The Age of Vulnerability: Eisenhower and the National Insecurity State," pp. 963–89.

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In This Issue

During a period of rapidly changing relations between the Soviet Union and the United States, historians are reexamining earlier interactions between the two countries. **Eduard Mark** argues that American cold war policies did not develop out of an unbroken tradition of anti-Bolshevism. Aware of the egalitarian and internationalist goals of the Russian revolution, many Americans, not just those on the left, attempted to explain Stalin's abandonment of those ideals. By the mid-1930s, four different interpretations of Stalinism could be found in the American press. Although all four interpretations (Mark calls them paradigms) informed and divided public opinion for over a decade, the relative influence of each changed as Soviet actions enhanced or diminished its credibility. By the late 1940s, two interpretations, the National Bolshevik (preferred by the American Right) and the Totalitarian (preferred by the American Left), converged to form the most convincing explanation of the ideologically recharged and expansionist Stalinism of the postwar years and thus served to unite the country behind a policy of containment.

H. W. Brands looks at a later stage in the development of the United States' cold war policy: the Eisenhower years. Brands challenges the revisionist view of Eisenhower as a skillful chief executive, contending that the president, after a promising start, was overwhelmed by opposition from the American national-security bureaucracy and by the daunting character of the problems new technologies posed to American security. Eisenhower entered office hoping that the latest developments in weapons design would restrain defense spending and render the United States secure from enemy attack, without jeopardizing the country's economic well-being, but he proved unable to control the emerging military-industrial complex. At his departure from office, large defense budgets were a near certainty for the foreseeable future, and Americans felt more insecure, not less.

Two very different sorts of review essays follow. The first focuses on a single book, Peter Novick's important new study of the American historical profession, *That Noble Dream: The "Objectivity Question" and the American Historical Profession*. **James T. Kloppenberg** praises Novick's book as a richly detailed social history showing how the philosophical positions of leading historians shaped the development of the discipline. Kloppenberg criticizes Novick for not elaborating his own views on the issues that he surveys. Kloppenberg also questions Novick's sharp dichotomy between the ideal of objectivity and a vague, relativistic alternative and contends that a large number of historians adopted intermediate positions. He adds his own voice to this debate by advocating one of these positions, an approach he terms "pragmatic hermeneutics," a conception of historical inquiry as a skeptical, open-ended social practice.

Maurice Isserman looks at five recent histories of the American New Left in the 1960s, all but one written by participants in that movement. Isserman offers a provocative overview that presents the Students for a Democratic Society (SDS) as the radical heart of the New Left. He warns that the books under review focus almost exclusively on national leaders and events. Only local and regional studies can help us understand such important outgrowths of the political left of the 1960s as the contemporary women's movement.

The editors and the Board of Editors for several years have been concerned about the responsibility of the *AHR* for the critical evaluation of historical representations on film. In December 1988, we featured an *AHR Forum* on some theoretical issues raised by the use of films to explicate and teach history. With this issue, we begin what we expect to be an annual review of historical films. The first set of reviews is the work of **Robert A. Rosenstone**, who identified the films and selected the reviewers. He points out in his introduction that he chose to begin with films covering a wide range of subjects and geographical areas.

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October or Thermidor? Interpretations of Stalinism and the Perception of Soviet Foreign Policy in the United States, 1927–1947

EDUARD MARK

AFTER THE DEATH OF V. I. LENIN in 1924, Joseph Stalin and Leon Trotsky vied for mastery of the Soviet Union. Stalin sealed his victory by expelling his rival from the Communist party in 1927. The next year, he decreed the First Five-Year Plan and the collectivization of agriculture. These initiatives surprised those who had thought him a moderate for having championed “socialism in one country” against Trotsky’s “permanent revolution.” But much else Stalin wrought seemed to confirm his relative conservatism. In 1931, he denounced the equalization of wages as “leveling.” Piecework, which Karl Marx had condemned, became common; the bureaucracy of party and state soon seemed a “new class.” There was a general rehabilitation of what a scoffing American Marxist called “old cultural barbarisms”: Great Russian nationalism became a theme of domestic propaganda in 1934; the progressive pedagogy and artistic innovation of the 1920s were suppressed, abortions banned, and divorce made difficult.¹ Stalin and lesser spokesmen abjured support for proletarian movements abroad; under “socialism in one country,” they explained, the USSR served world revolution only as an example for others to follow.²

During the 1920s, many commentators had confidently predicted the ultimate failure of the “Soviet Experiment.” But, except for short-lived speculation after the announcement of the New Economic Policy in 1921, there had been few claims that Bolshevism had already ceased to be socialistic. Whatever their feelings about socialism, Americans generally agreed that it implied a society that

This paper is based on nearly nine hundred books about the USSR published between 1925 and 1950, forty representative newspapers for the period 1927–1947, all articles dealing with the Soviet Union listed in standard periodical indexes between 1927 and 1950, and most declassified records of the Executive Branch relating to the USSR, 1927–1947. As the space for notes is limited, general points are supported by only a few representative citations. On request, I will make available to interested readers a more extensive list of citations and the results of the survey of editorial opinion cited in the notes.

¹ Benjamin Stolberg, “October into Thermidor,” *Nation*, 144 (April 10, 1937): 402. For Stalinism as a historical problem, see Robert C. Tucker, ed., *Stalinism: Essays in Historical Interpretation* (New York, 1977).

² See, for example, Karl Radek, “The Bases of Soviet Foreign Policy,” *Foreign Affairs*, 12 (January 1934): 192–206.

was not only collectivist but also equalitarian, internationalist, and culturally “progressive.” Bolshevism had represented these values credibly.³ The later trend toward inequality, nationalism, and cultural conservatism could hardly fail, therefore, to raise questions about the fate of the Russian revolution at Stalin’s hands. By the mid-1930s, there were four distinctly different explanations of what was increasingly called “Stalinism.”⁴ These constructions reflected the divergent ideological preconceptions and political interests that Americans brought to their attempts to explain the growing discrepancies between Soviet practice and the traditional values of socialism.

Virtually the only point of agreement between the various schools of interpretation was that elemental changes had indeed taken place in the USSR. In Lenin’s day, world revolution had been more or less synonymous with proletarian insurrection. The threat of communism, as the Right had conceived it in the 1920s, had chiefly resided in the Soviet Union’s propaganda and its alleged subvention of native radicals. Growing Soviet “nationalism” and successive Five-Year Plans, however, prompted some conservatives to redefine the danger. They argued that, since the world had shown so few signs of spontaneous revolt, Stalin planned to make the Red Army the great engine of communization.⁵ But this vision of a new-model world revolution (I shall call it the National Bolshevik Paradigm) faltered as the dictator ever more dramatically altered the Bolshevik

³ For useful overviews of opinion about the USSR during the 1920s, see Peter G. Filene, *Americans and the Soviet Experiment, 1917–1933* (Cambridge, Mass., 1967); and Paul Scheffer, “Points of View about the Soviet Union,” *Asia*, 30 (July 1930): 488 and following.

⁴ I use “Stalinism” in its original sense—to denote the sum total of the changes that distinguished Stalin’s communism from Lenin’s. I plead agnosticism in the current debate among students of Soviet Communism about whether they represented a repudiation of Bolshevism or were in some sense a fulfillment of it. It should be noted that, in the 1930s, the term “Stalinism” had no particular emotional valence. It was, for example, used by Trotskyists, clearly pejoratively, but also by the likes of Walter Duranty, Moscow correspondent of the *New York Times*, who was (to say the least) well disposed to Stalin.

⁵ The terms “conservative,” “liberal,” and “radical” are used quite conventionally in this paper. It may nonetheless be helpful to define them in relation to those tenets and practices of early Bolshevism—economic collectivism, equalitarianism, internationalism, progressive education, sexual freedom, and the emancipation of women, to name the more salient—that were taken as indexes of Stalin’s divergence from Lenin. Radicals—chiefly socialists or communists of one persuasion or another—quite uniformly embraced both the socio-cultural and the economic program of Bolshevism, although some of them harbored strong reservations about Soviet methods. Conservatives, champions of private property, minimal government, and traditional proprieties and allegiances condemned all dimensions of Bolshevism. Liberals were well disposed to the socio-cultural aspects of Bolshevism but tended not to endorse the complete socialization of industry. They did, however, generally admire the Soviets’ attempts to plan for social and economic progress. What seems to have most drawn liberals to Soviet Communism was its effort to subject society to rational direction. There are many fine studies of liberal and radical thought between the wars; conservatism has not been so well explored. I have found the following works useful: Clinton Rossiter, *Conservatism in America: The Thankless Persuasion* (New York, 1962), 20–66, 128–96; Arthur Schlesinger, Jr., *The Age of Roosevelt: The Crisis of the Old Order* (Cambridge, Mass., 1957), 54–89, 130–44; Arthur Schlesinger, Jr., *The Age of Roosevelt: The Politics of Upheaval* (Cambridge, Mass., 1960), 147–207, 385–408; Frank A. Warren, *The Alternative Vision: The Socialist Party in the 1930s* (Bloomington, Ind., 1974), 3–20, 123–57; Irving Howe and Lewis Coser, *The American Communist Party: A Critical History (1919–1957)* (Boston, 1957), 1–40, 144–386; John P. Diggins, *The American Left in the Twentieth Century* (New York, 1973), 3–152; Alonzo L. Hamby, *Beyond the New Deal: Harry S. Truman and American Liberalism* (New York, 1973), xiv, 9–11; Lewis S. Feuer, “American Travelers to the Soviet Union, 1917–32: The Formation of a Component of New Deal Ideology,” *American Quarterly*, 14 (Summer 1962): 119–49; Frank A. Warren, *Liberals and Communism: The “Red Decade” Revisited* (Bloomington, Ind., 1966), 1–88.

heritage. Conservatives increasingly portrayed Stalinism as a Thermidorian reaction, the first stage of an inevitable capitalist restoration that mocked the hopes of liberals and radicals for social progress (the Thermidorian Paradigm). Partly to rebut such claims, but also to defuse the Right's persistent tactic of red-baiting, many liberals explained Stalinism as an evolutionary development of Bolshevism that, while shorn of its wilder extremes, remained a program for social betterment (the Evolutionary Paradigm). Many noncommunist radicals, equally loathe to concede the inevitability of the old order but anxious that Stalinism should not be taken for socialism, helped to propound the theory of totalitarianism, which held that Stalinism exemplified a third path for modern society, neither capitalist nor socialist (the Totalitarian Paradigm).

Historians have too little appreciated that, to understand American perceptions of Soviet Communism during and after World War II, it avails little to know what Americans thought of Marxism in the abstract or how they greeted the Russian revolution of 1917. By 1939, few in the United States doubted that Stalinism had supplanted Bolshevism, even though there was no consensus about the nature of the new Soviet dispensation. The four interpretations of Stalinism continued to inform—and to divide—American perceptions of Soviet Communism during World War II and the cold war. They changed little during those years: the Stalinist system had assumed its final form by the mid-1930s, and the standpoints from which Americans viewed it remained essentially constant. What did change was the degree of support that each interpretation of Stalinism found relative to the others. All were ultimately subject to the test of explaining and predicting Soviet behavior. The process by which the several interpretations of Stalinism were first postulated and then weighed against events is complex but rewarding to trace. It explains the relative optimism with which Americans viewed the USSR during the war and makes more comprehensible their later fears of that state.

FROM THE FIRST, THE POLITICAL SIGNIFICANCE of the "Soviet Experiment" largely derived from its seeming a test, not only of socialism but also of whether society was to any great extent subject to conscious improvement. Conservatives had long appealed to the Christian tenet of original sin to argue that collectivism must fail because it denied the immutable selfishness of human nature. In Social Darwinism, the infant science of genetics, and the instinctual psychologies of the early century, they found newer arguments that the world was much as it had to be. Liberals and radicals took a contrary view, staking their hopes for social change on the increasingly dominant environmentalism of the social sciences.⁶

With the advent of Stalinism, many on the Right were quick to claim vindication in what the *Literary Digest* called Russia's "backward trek to the safeties and sanities proved by social experience." Ambassador Joseph E. Davies

⁶ Merle Curti, *Human Nature in American Thought: A History* (Madison, Wis., 1980), 217–312; John Dewey, *Human Nature and Conduct: An Introduction* (New York, 1950), 106–07.

expressed a common opinion when he reported from Moscow in 1937 that there had been a “Thermidor” because “theoretical communists, when clothed with responsibility,” had been “compelled to resort to the elementals of human nature.” Advocates of the Thermidorian Paradigm seized on the USSR’s growing inequality and socio-cultural conservatism to argue opportunely, if not insincerely, that Stalinism marked an inevitable reversion to “natural” socioeconomic norms, even to capitalism. Was it not odd, asked the editor of *Nation’s Business* in 1936, that even as “capitalism” was proving its worth in Russia, it was being relegated to “economic limbo” in the United States “as fast as the public planner can think up ways to get it there?”⁷ Much of the Right saw in the fate of the Old Bolsheviks during the purges of 1936–1938 proof of what the *Chicago Tribune* called “the final liquidation of the Red Revolution.”⁸

Since their philosophy stressed nurture, liberals were not disposed to expect that nature would reclaim revolutionaries with a Thermidor. Repelled by Trotsky’s advocacy of violent revolution, they were quick to hail “socialism in one country” as proof of Stalin’s sensibleness. “Whatever their enthusiastic leaders may have thought a few years ago,” the *New Republic* stated in 1931, the Soviets now appreciated “that radicalism is a native plant which must grow . . . at its own rate and can be forced very little if at all.”⁹ However liberals judged the purge trials, they saw them as the result of a struggle between what the *Nation* called “two theories of revolution”—Stalin’s and that of “unreconstructed Bolsheviks” who had seen in the evolution of the USSR from a “center of revolution and propaganda” into a “great power” a “treasonable surrender of revolutionary purposes and principles.”¹⁰

Those radicals whose admiration for the “Soviet Experiment” had predated the Great Depression had been as much attracted to Bolshevism’s promise of social and cultural emancipation as to its collectivism. By the mid-1930s, many of them had concluded, as Max Eastman wrote in 1937, that the USSR would soon be “as reactionary as any state which has emerged from feudalism.” Liberals, on the other hand, social engineers rather than romantic revolutionaries, by and large shared neither the equalitarianism nor the bohemianism of the radicals. They had been little interested in the Soviet Union before 1929. It was primarily the presumed efficiency of centralized economic planning—a policy closely associated with Stalin—that subsequently drew their attention to the USSR. Few

⁷ “Russia Rediscovered Family Life’s Value,” *Literary Digest*, 120 (July 13, 1935): 20; Joseph E. Davies to Cordell Hull, Record Group 59, General Records of the Department of State, 861.20/425, National Archives, Washington, D.C.; “Through the Editor’s Specs,” *Nation’s Business*, 24 (July 1936): 7. Conservatives had briefly invested similar hopes in the New Economic Policy of 1921. Filene, *Americans and the Soviet Experiment, 1917–1933*, 102–10.

⁸ *Chicago Tribune* (March 1, 1938): 10. See also “Eight Dead Dogs,” *Time*, 24 (June 21, 1937): 23–24; *Des Moines Register* (September 11, 1936): 6; *New York Daily News* (January 26, 1937): 25. Of forty newspapers I selected for political and geographical representativeness, 89 percent concluded that the purges marked a break with Bolshevism.

⁹ Bruce Bliven, “Russia in Hope,” *New Republic*, 69 (December 2, 1931): 60–62. See also “Litvinov’s London Triumph,” *ibid.*, 303–04; Oswald Garrison Villard, “Russia from a Car Window: The Spirit of Government,” *Nation*, 129 (November 20, 1929): 576–79.

¹⁰ “Behind the Show Trials,” *Nation*, 144 (February 7, 1937): 143–45; “Russia and the World,” *ibid.* (November 13, 1937): 521. See also “The Trial of the Trotskyites in Russia,” *New Republic*, 88 (September 2, 1936): 88–89.

liberals, Norman Thomas complained, understood that socialism had ever meant "something more than an elaborate system of state ownership."¹¹

Liberals therefore tended to deny the charges by "businessmen and radicals" that Soviet socialism was "in collapse." Stalin's encouragement of family life and patriotism, both the objects of radical scorn, seemed to many liberals proof only of a commendable moderation.¹² The Soviet Union was "evolving," wrote the eminent Samuel N. Harper of the University of Chicago, but always "in line with its revolutionary program."¹³ The Evolutionary Paradigm's vision of Stalinism as a faithful but subdued offspring of Bolshevism appealed to liberals in three ways: it reconciled their reformism with an attraction to communism, while providing a theoretical basis for the Popular Front against Fascism and a refutation of conservative charges of a Soviet Thermidor designed to discourage social change in the United States.

The belief that Bolshevism had moderated was not universal. But Stalin's well-publicized departures from Bolshevism soon compelled those who stressed the primacy of ideology in the USSR to redefine the communist threat.¹⁴ The National Bolshevik Paradigm postulated that the Soviet state, rather than the national proletariats, had become the chief instrument of world revolution. Income differentials could thus be reconciled with communism because they promoted the industrial efficiency to forge a modern military, nationalism because it galvanized the Russian masses as Marxism could not. In 1932, the American minister in Riga, Latvia, asked his staff whether the USSR had not advanced far "toward the kind of nationhood that we can and should recognize." His first secretary disagreed, warning that the change was but "an enforced hibernation" until Russia was "so financially, industrially, and militarily strong as to warrant a great attempt to communize the world by arms."¹⁵

Such views became steadily less common. The USSR's renunciation of revo-

¹¹ Max Eastman, "The End of Socialism in Russia," *Harper's*, 174 (February 1937): 302; Warren, *Liberals and Communism*, 65, 88, 104–07, 116–17; Norman Thomas, *Socialism on the Defensive* (New York, 1938), 31.

¹² See, for example, Maurice Hindus, "Russia Grows Up," *Harper's*, 174 (May 1937): 611–20 (quoted); Louis Fischer, "Moscow Frowns on Abortions," *Nation*, 161 (August 21, 1935): 212–13; John Gunther, *Inside Europe* (New York, 1937), 446–48.

¹³ Samuel N. Harper, *The Government of the Soviet Union* (New York, 1938), 195. See also Walter Duranty, *I Write as I Please* (New York, 1935), 178–81, 338–40; Joseph Barnes, "World Revolution 1934 Model," *Asia*, 34 (September 1934): 585–89; Gunther, *Inside Europe*, 413–15, 453–56; Harry F. Ward, *In Place of Profit* (New York, 1933), viii, 389–90.

¹⁴ Three groups are salient: the Catholic hierarchy, militant conservatives, and academics who stressed the enduring influence of ideas—in this case, those of Marx and Lenin. The Reverend Edmund A. Walsh, vice-president of Georgetown University, was in effect the church's official spokesman on matters Soviet. See his *The Last Stand: An Interpretation of the Soviet Five Year Plan* (Boston, 1931), 169–70. Warnings of the unchanging nature of Soviet objectives were not uncommon in conservative publications before 1937–1938 but were nowhere voiced more consistently than in the newspapers owned by William Randolph Hearst. The following is typical: *San Francisco Examiner* (January 7, 1937): 10. For an example of an academic study based on the patristic writings of the communist movement, see T. A. Taracouzio, *War and Peace in Soviet Diplomacy* (New York, 1940).

¹⁵ Robert F. Skinner to Henry L. Stimson, March 24, 1932, enclosing "Reflections on Russia" and Felix D. Cole's comments thereon, RG 59, General Records of the Department of State, 861.01/1742. See also Joseph E. Davies to Cordell Hull, March 17, 1937, enclosing memorandum by Loy W. Henderson, *ibid.*, 861.00/11496; Walsh, *Last Stand*, 130; Lawrence Dennis, "Soviet Russia Goes on Sale," *American Mercury*, 39 (December 1936): 469–78.

lutionary interventionism, its championship of disarmament, and the many nonaggression pacts it signed with neighboring states made a favorable impression in the United States, as did its championship of collective security upon joining the League of Nations in 1934. By the mid-1930s, the press commonly treated the USSR as a status-quo power. No state, the *New York Times* declared in 1936, had been “more conciliatory.”¹⁶ Conservatives continued to rail at the danger of communism in the United States. But communism in Russia was a separate issue, and their interests were well served by portraying Stalin as a man whose pragmatism belied the collectivist schemes of their opponents. As the pattern of Stalin’s rule unfolded, moreover, it became increasingly difficult to impute transcendent purposes to it. The effect of the purges on diplomatic reporting is a case in point. The Moscow embassy was originally partial to the National Bolshevik Paradigm. But, when it became apparent that the purges were weakening the USSR, the supposed engine of revolution, diplomats began to stress the preservation of the regime as an end in itself. “The communist state,” the embassy reported early in 1939, “as based on the principles of revolution, has revolved into a dictatorship as based on the personal power of Stalin.”¹⁷

A stress on dictatorship as an end in itself characterized the fourth paradigm of Stalinism, the Totalitarian. When both Germany and Italy resorted to command economies and rhetorical anticapitalism, it suggested to many that fascism was not simply capitalism *in extremis*. Benito Mussolini, for example, declared the eventual nationalization of industry in 1937, having not long before told an American journalist that fascism and communism were essentially the same.¹⁸ The supposed shift of Soviet Communism to the “right” and of fascism to the “left” fostered the idea that both exemplified a third path for industrial society—bureaucratic collectivism, or totalitarianism. There were many explanations of the phenomenon. But all agreed that it favored planning and collectivism over the free market and private property of capitalism and opposed dictatorship, hierarchy, and nationalism to the democratic equalitarianism and internationalism of socialism.¹⁹

The theory of totalitarianism proved attractive to noncommunist radicals and those liberals who, like John Dewey, had never reconciled themselves to Soviet methods. Much of the democratic Left embraced it as a means of distinguishing socialism from communism that conceded nothing to the Right’s charges that

¹⁶ *New York Times* (April 24, 1936): 20. For a useful survey of press opinion at mid-decade, see Thomas R. Maddux, “Red Fascism, Brown Bolshevism: The American Image of Totalitarianism in the 1930s,” *Historian*, 40 (November 1977): 94–95; Thomas R. Maddux, *Years of Estrangement: American Relations with the Soviet Union, 1933–1941* (Tallahassee, Fla., 1980), 156, table 4.

¹⁷ *Foreign Relations of the United States: The Soviet Union, 1933–1939* (Washington, D.C., 1952), 753; for the changing emphasis, see 225, 300–01, 310–11, 380–81, 378, 384–85, and note 101 below.

¹⁸ Alfred Bingham, *Insurgent America: The Revolt of the Middle Classes* (New York), 165.

¹⁹ See, for example, James Burnham, *The Managerial Revolution: What Is Happening in the World* (New York, 1940), 20–75, 123–71, 208–22; Calvin B. Hoover, *Dictators and Democracies* (New York, 1937), vii, 41–62, 72–76; William Henry Chamberlin, *Collectivism: A False Utopia* (New York, 1937), 22–63, 245–47; Walter Lippmann, *An Inquiry into the Nature of the Good Society* (Boston, 1937), x–xi, 3–19, 45–90, 103–05, 139–41, 168–73, 205–06, 234–35.

Stalinism had proved the inevitability of capitalism.²⁰ A growing number of conservative intellectuals, among them Walter Lippmann and Herbert Hoover, also adopted the Totalitarian Paradigm. By the late 1930s, it was clear to informed observers that Stalinism was not even nascently capitalistic. The theory of totalitarianism afforded an alternative means of arguing, as Hoover wrote in 1937, that the Soviets had proved “that Socialism will not work.” But, while the Left attributed Soviet totalitarianism to Lenin’s undemocratic methods, conservatives described it as the unintended consequence of the same economic planning that liberals advocated for the United States.²¹

It has been assumed that, to the extent that the Soviet system was held to be totalitarian, the USSR’s foreign policy was equated with those of Germany and Italy.²² It is more accurate to say that the Soviet Union’s putative totalitarianism encouraged suspicions of its potential aggressiveness, which were kept in check before the cold war by the belief that the USSR differed in important respects from the other totalitarian powers. The propaganda of all the dictatorships was chronically militant and made a calculated use of enemies to justify tyranny—Hitler warned of the Jews, Stalin of the “capitalist encirclement.” Reckless actions by Italy and Germany, particularly the invasion of Ethiopia in 1935 and the remilitarization of the Rhineland the next year, suggested that those countries, at least, needed not only the appearance but the reality of conflict. As both states were clearly unready for general war, internal considerations—especially the need to distract attention from intractable economic problems—plausibly explained their rashness.²³ The Soviets, however, were largely exempt from this speculation—partly because of their irenic diplomacy but also because of the great influence of a geopolitical theory that the principal causes of aggression were overpopulation and shortages of natural resources. Germany and Italy were, as they themselves proclaimed, “have-not” nations. But the USSR possessed an incomparable natural endowment and was if anything underpopulated. Thus *Fortune*, no friend to communism, said of the Soviets that the “usual causes which drive nations to war do not exist for them.”²⁴

Because of this exemption, the Totalitarian Paradigm, no less than the Evolutionary and Thermidorian, tended to reinforce the idea that the USSR was

²⁰ See, for example, Thomas, *Socialism on the Defensive*, 13–17; Agnes B. Meyer, “Significance of the Trotsky Trial: An Interview with John Dewey,” *International Conciliation*, 337 (February 1938): 53–61; Sidney Hook, *Reason, Social Myths, and Democracy* (New York, 1940), 146–49, 164 n. 165–76.

²¹ Herbert Hoover, *Addresses upon the American Road, 1933–1938* (New York, 1938), 317, 323; Lippmann, *Inquiry into the Principles of the Good Society*, 3–7, 50–53, 56.

²² See, for example, Les K. Adler and Thomas G. Paterson, “Red Fascism: The Merger of Nazi Germany and Soviet Russia in the American Image of Totalitarianism, 1930s–1950s,” *AHR*, 75 (April 1970): 1046–64.

²³ See, for example, *New York Herald Tribune* (July 3, 1935): 12; and *ibid.* (July 15, 1935): 14; *Washington Post* (August 20, 1936): 8; *St. Louis Post-Dispatch* (July 16, 1935): 2C; *Chicago Daily News* (September 11, 1936): 16.

²⁴ “Background of War: USSR,” *Fortune*, 16 (August 1937): 70–71 and following. See also Lippmann, *Inquiry into the Principles of the Good Society*, 145–46; Chamberlin, *Collectivism: A False Utopia*, 50. For the broad influence of the “have-not” theory of aggression, see Hans Kohn, *Living in a World Revolution: My Encounters with History* (New York, 1964), 26–27. For its effect on British and American diplomats trying to “appease” Germany, see C. A. MacDonald, *The United States, Britain, and Appeasement* (New York, 1980), 1–14, 62–88, 106–07, 150–51.

becoming a conventional great power. Proponents of the Thermidorian Paradigm saw in the Soviet Union's reversion to nationalism a manifestation of the conservative tenor of Stalinism. The *New York Times*, which as early as 1931 had referred to Stalin's "Russian sinn feinism," opined in 1936 that there was "at least a reasonable chance that Russian national interests rather than Communist world interests" would continue to guide Soviet policy.²⁵ The Evolutionary Paradigm, predicated on "socialism in one country," suggested the same. Samuel N. Harper wrote that, "while the Bolsheviks started off in a crusading spirit, working for world revolution, they have in fact set up a system that manifests in its relations with other countries the characteristics of a national state" that needed peace to fulfill its mission of "'propaganda by example.'"²⁶ According to the Totalitarian Paradigm, the Soviet Union was becoming nationalistic because the personal interests of Stalin and his bureaucratic minions were invested in the Soviet state. Their lodestar, like that of other settled elites, was not ideology but self-perpetuation. The USSR, wrote Sidney Hook, a leading exponent of the Totalitarian Paradigm, was "irrevocably a part of the comity of nations, the staunchest defender of the European status quo."²⁷

The idea of Russia redux was to be the first premise of American policy toward the Soviet Union during World War II. It was, ironically, only strengthened by events that brought upon the USSR more opprobrium than any since 1917—the nonaggression pact with Germany of August 23, 1939, the subsequent partition of Poland, and, particularly, the attack on Finland in December 1939. Together with the defeatism of the Western communist parties and *Izvestiia's* famous declaration that fascism was "a matter of taste," these actions seemed to bespeak a cynicism incompatible with ideological commitment. "The mask is dropped," declared a typical editorial after the pact, "and the ideologies are seen for what they are—the hypocritical shields by means of which prideful dictators have played imperial politics."²⁸

Most commentators saw Stalin's actions either as a defensive, if ruthless, effort to shore up Soviet defenses or as nationalistic freebooting.²⁹ One line of speculation held that the weakness of his purge-racked state had led him to become a German satrap, exchanging raw materials for the technical expertise to preserve a faltering regime. This theory figured in diplomatic reporting from Moscow, which stressed the defensiveness of Stalin's diplomacy and its minimal ideological content. Charles E. Bohlen, then emerging as one of the diplomatic

²⁵ *New York Times* (December 1, 1931): 28; *ibid.* (April 24, 1936): 20.

²⁶ Samuel N. Harper, "The Soviet Union—National or International?" *Annals of the American Academy of Political and Social Science*, 175 (September 1934): 58–59.

²⁷ Sidney Hook, "The Prophetic Trotsky," *Saturday Review of Literature*, 15 (July 1936): 10.

²⁸ *Hartford Courant* (August 23, 1939): 8. For useful surveys, see Meno Lowenstein, *American Opinion of Soviet Russia* (Washington, D.C., 1941), 156–57; Maddux, *Years of Estrangement*, 102–06.

²⁹ My survey of editorial opinion found that 71 percent of the papers ascribed strictly nationalistic motives to Soviet expansionism, and 20 percent ideological motives—that is, world revolution. For a similar finding, see Maddux, *Years of Estrangement*, 167, table 5.

corps' most influential Soviet specialists, wrote in April 1940 that "Marxian ideology" had from "all evidence ceased to be a factor" in Soviet diplomacy.³⁰

Neither was there much disposition to attribute Soviet expansionism to the internal exigencies of totalitarianism.³¹ There was, however, a widespread impression that Britain and France had spurned sincere Soviet offers of alliance and at Munich encouraged Hitler's *Drang nach Osten*.³² It seemed possible that Stalin had signed with Germany to avoid a war with dubious allies and then expanded his frontiers against German treachery. This theory grew more plausible once Hitler's victories made his power seem to be the equal of his ambition. A journalist noted late in 1940 that "the opinion is gaining that, insofar as Russia's motives were aggressive, they were of a defensive character."³³ Most journalistic and diplomatic commentary on the seizure of the three Baltic states in the summer of 1940 stressed considerations of defense; there was little talk of communization for its own sake as even a secondary aim. When Hitler struck eastward in June, 1941, the Gallup poll found that Americans overwhelmingly favored the USSR, believing that the Soviets, unlike their assailants, were "not imperialistic."³⁴

NEITHER THE LATER COURSE OF THE WAR nor the Soviet-American alliance of 1941 occasioned a revision of the American interpretations of Stalinism. The "conservative" trends in Soviet society grew more pronounced. Of particular interest in the United States was the use of Great Russian nationalism to mobilize resistance to the German invaders.³⁵ The needs and preconceptions that Americans brought to their observation of Soviet Communism also remained basically the same. It was, then, predominantly to the Thermidorian, Evolution-

³⁰ Laurence Steinhardt to Cordell Hull, April 15, 1940, enclosing memorandum by Charles E. Bohlen, RG 59, General Records of the Department of State, 761.621/661. See also Eugene Lyons, "Must Stalin Fight Hitler?" *American Mercury*, 52 (January 1941): 24-32; Demaree Bess, "Nazi Germany's First Dominion," *Saturday Evening Post*, 212 (May 18, 1940): 31-32; Burnham, *Managerial Revolution*, 224-26.

³¹ I have found only two commentaries that attributed Soviet expansion to the internal imperatives of dictatorship: *Christian Science Monitor* (December 4, 1939): 18; Louis Fischer, "Soviet Russia Today: The Death of a Revolution," *Nation*, 130 (January 13, 1940): 37-40. Moral comparisons of Stalin's actions with Hitler's were common. But that was not the same as saying that their motives were the same.

³² At the time of the Russo-German pact, for example, there was fairly widespread sentiment that Britain and France had been repaid in kind. See, for example, *Boston Globe* (August 26, 1939): 10; *Chicago Daily News* (September 7, 1939): 16; *New York Daily News* (August 25, 1939): 25; *St. Louis Post-Dispatch* (August 22, 1939): 2C.

³³ Andrew J. Steiger, "Soviet Russia Needs No War," *Asia*, 40 (November 1940): 573-75. My survey of editorial opinion found that 59 percent of the papers ascribed Soviet actions to security and 22 percent to nationalistic imperialism, while only 15 percent mentioned ideology as even a secondary factor. For a similar result, see Maddux, *Years of Estrangement*, 167, table 5. Official reporting also stressed Soviet weakness and defensiveness. See, for example, *Foreign Relations of the United States: Diplomatic Papers, 1940*, 5 vols. (Washington, 1955-61), 1: 470-72, 3: 211-15.

³⁴ *New York Times* (July 13, 1941): 2.

³⁵ For typical résumés, see Cyril Black, "War Changes in Russia," *Current History*, 2 (June 1942): 283-86; William Henry Chamberlin, "Sources of Russia's Strength," *Harper's*, 186 (March 1943): 396-403.

ary, and Totalitarian paradigms that Americans looked for answers to pressing questions posed by alliance with the USSR.³⁶

The intellectual hegemony of the established interpretations of Stalinism was reflected by polls showing that only 15 percent of the public believed the Soviets wanted to “spread Communism” and that even mistrust of the USSR was rarely based on its ideology.³⁷ A leading academic authority on the Soviet Union observed that, because of “the internal evolution of the Soviet regime,” most of his colleagues believed a “resurgence of revolutionary interventionism” to be, of all courses for the USSR, “the least likely.” Study groups of scholars and community leaders convened across the country by the Carnegie Endowment and the Council on Foreign Relations agreed.³⁸ Official analyses mostly portrayed the Soviet Union as a postrevolutionary society. “A deeply and consciously cultivated national patriotism,” the Moscow embassy reported early in 1944, was “the most dynamic element in the Soviet scene.” Marxism, by contrast, was “an honored tradition rather than a living philosophy.”³⁹ Diplomatic calculations were based on the conclusion that “the foreign policy of the USSR, like that of any other established and stabilized world power,” was “controlled primarily by considerations of national interest.”⁴⁰

Conservatives, buoyed by wartime prosperity and the rehabilitation of business as the “arsenal of democracy,” mounted a determined counterattack on the New Deal after Republicans gained in the election of 1942. They continued to use the Thermidorian Paradigm to confirm their vision of the world—and to portray liberals as leftists more doctrinaire than Soviet Communists. In 1944, the *New York Times* noted with satisfaction that in Russia “the capitalistic system,

³⁶ There has been virtually no appreciation of the continuity of wartime thought about Soviet Communism with that of the 1930s. Assertions that the Soviet Union had fundamentally changed, when noted, have been unfailingly ascribed to some form of wishful thinking. See, for example, Ralph Levering, *American Opinion and the Russian Alliance* (Chapel Hill, N.C., 1976), 203; John Lewis Gaddis, *The United States and the Origins of the Cold War, 1941–1947* (New York, 1972), 31, 51–62; Robert Dallek, *The American Style of Foreign Policy: Cultural Politics and Foreign Affairs* (New York, 1983), 154–86; Hugh De Santis, *The Diplomacy of Silence: The American Foreign Service, The Soviet Union and the Cold War, 1933–1945* (Chicago, 1979), 101. Other scholars have assumed that officials continued to see the Soviet Union as the embodiment of world revolution. See, for example, Gabriel Kolko, *The Politics of War: The World and United States Foreign Policy, 1941–1949* (New York, 1968), 184; Lloyd C. Gardner, *Architects of Illusion: Men and Ideas in American Foreign Policy, 1941–1949* (Chicago, 1970), ix; Daniel Yergin, *Shattered Peace: The Origins of the Cold War and the National Security State* (Boston, 1978), 39–41.

³⁷ Warren B. Walsh, “What the American People Think of Russia,” *Public Opinion Quarterly*, 8 (Winter 1944): 514; Department of State, “Public Attitudes on Foreign Policy,” no. 24 (May 30, 1944), RG 59, Records of the Office of Public Opinion Studies, box 1.

³⁸ Michael Karpovich, “Russia in the New World,” in Ruth Nanda Anshen, ed., *Beyond Victory* (New York, 1943), 117; Percy Bidwell, ed., *The United States and the United Nations: Views on Postwar Relations* (New York, 1943), 16–31; “Universities Committee on Postwar Problems: Problem XIV, The Soviet Union,” *International Conciliation*, 310 (April 1945): 261–73.

³⁹ W. Averell Harriman to Cordell Hull, January 5, 1944, enclosing “Survey of Domestic Developments in the Soviet Union in 1943: RG 59, General Records of the Department of State, 961.00/12045. See also Maxwell Hamilton to Cordell Hull, October 2, 1943, *ibid.*, 861.01/2277; George F. Kennan to Cordell Hull, July 12, 1944, *ibid.*, 861.9111/7–1244.

⁴⁰ Inter-Divisional Subcommittee on Latin America Country Problems, “The Objectives of Soviet Policy with Respect to Latin America,” August 24, 1944, RG 59, Records of Harley A. Notter, box 112. See also Policy Committee, Document PC-4, “War and Peace Aims of the Soviet Union,” March 29, 1944, *ibid.*, box 138; Charles E. Bohlen, “United States-Soviet Relations, 1933–1943, December 11, 1944, RG 59, Records of Charles E. Bohlen, box 8.

better described as the competitive system, is back.” It then attacked the War Labor Board for being “reluctant to accept the principle of incentive wages.” The paper noted Soviet use of piecework to argue that it was “a particularly strange paradox of our time” that the United States promoted equality of wages while “the ‘fatherland of socialism’ should be reverting to the capitalistic principle of wage payments on the basis of productivity.”⁴¹ It was no anomaly that, of all socioeconomic groups, business executives were the most optimistic about postwar relations with the USSR.⁴²

Shocked by Stalin’s collaboration with Hitler, some liberals charged what they had earlier denied: communism and fascism were both “totalitarian.”⁴³ After 1941, however, most re-embraced the Evolutionary Paradigm. This reaffirmation was no less expedient than the conservative espousal of the Thermidorian and Totalitarian paradigms, though not for that reason insincere. To admit that the Soviet Union was totalitarian could only lend force to conservative warnings about the dangers of planning—and at a time when many liberals were arguing that it was not coincidental that a nation with a planned economy had been the first to withstand the Nazi juggernaut.⁴⁴

In the conservative resurgence, moreover, liberals saw dangers of postwar economic crisis, native fascism, and a third world war. These fears were grounded in the conviction that “monopoly capitalism” was prone to crisis, with fascism a possible recourse. Many liberals predicted that business, unable to cope with postwar reconversion, might impose a dictatorship justified by the dangers of communism at home or abroad.⁴⁵ They saw the first signs of this in the willingness of the Western Allies to deal with Fascists in North Africa and Italy. They denounced these moves, which they blamed on “reactionaries” in the Department of State, as harbingers of a new Holy Alliance against the worldwide revolt of the “Common Man.”⁴⁶ Liberals enlisted the Evolutionary Paradigm in their fight against America’s protofascists by using it to prove that communism was no threat: “In order to achieve the economic and social progress regarded by the Russians as necessary to the completion of their revolution which, unlike its French predecessor, knows no Thermidor, the Soviet Union needs peace.”⁴⁷

⁴¹ *New York Times* (April 3, 1944): 20; and (April 4, 1944): 20. Similarly, when the industrialist Eddie Rickenbacker returned to the United States from a visit to the USSR, he stressed pointedly that the Soviets were adopting “capitalism” and that they had “no labor problems,” *ibid.*, 1. See also remarks by Congressman John E. Rankin, *Congressional Record*, 78th Congress, 1st Session, 1943, 89, pt. 2 (Appendix), A3371; *New York Times* (December 22, 1943): 23. For a scholarly exposition of the Thermidorian Paradigm, see Pitirim Sorokin, *Russia and the United States* (New York, 1944), 178–209.

⁴² “U.S. Opinion on Russia,” *Fortune*, 32 (September 1945): 233–42.

⁴³ See, for example, “The Nazi-Soviet Partnership,” *New Republic*, 104 (May 20, 1941): 715–16.

⁴⁴ See, for example, *PM* (April 17, 1944): 2.

⁴⁵ Alonzo Hamby, *Beyond the New Deal: Harry S. Truman and American Liberalism* (New York, 1973), 5–7.

⁴⁶ See, for example, “Why Mr. Eden Is Here,” *New Republic*, 108 (March 29, 1933): 395–96; Freda Kirchwey, “Darlan and American Liberals,” *Nation*, 155 (November 28, 1942): 401–03; Irwin Ross, “America’s Palace Diplomacy,” *Common Sense*, 12 (June 1943): 192–93; Frederick L. Schuman, “The New Holy Alliance,” *Current History*, 6 (May 1944): 391–97.

⁴⁷ C. L. Sulzberger, “What Russia Is—What Russia Wants,” *New York Times Magazine* (January 28, 1945): 5–6 and following. See also Harold Fischer, “Stumbling Blocks and Stepping Stones,” *Survey Graphic*, 33 (February 1944): 123–24 and following.; “Russia’s Sphere of Influence,” *New Republic*,

Stalin, warned Freda Kirchwey of the *Nation*, would “make trouble” only if Anglo-American support for reactionary groups in Europe threatened Soviet security.⁴⁸

During the war, writers on both the Left and the Right continued to use the Totalitarian Paradigm for their separate purposes. Among conservatives, fears ran high that Franklin D. Roosevelt had plotted to get the United States into the war in order to perpetuate and to consolidate the New Deal. Many held up both the Soviet Union and the Fascist states as exhibits of what awaited America if it followed the path of planning and bureaucratism.⁴⁹ But liberals were the real target of these attacks, and mainstream conservatives usually evinced no great concern about the USSR, save for some uneasiness about revived Russian nationalism.⁵⁰ A much more alarming picture of Soviet totalitarianism came from social democrats who, as though incited by the new acceptance of Stalin, penned the sharpest attacks on the USSR to be found outside the extreme Right. Norman Thomas warned that “the Russian type of totalitarianism will remain a bureaucratic state capitalism, undemocratic, ruthless and Machiavellian.” He ridiculed the “curious confidence” that his fellow citizens had in Russian nationalism: “a nationalistic Russia will probably become aggressively imperialist.”⁵¹

On balance, however, the Totalitarian Paradigm was reassuring precisely because it postulated a nationalistic USSR. The journalist William Henry Chamberlin was probably the most respected American authority on the USSR after the death of Samuel N. Harper in 1943. When Roosevelt asked for a book about the Soviet Union on the eve of the Yalta Conference, the Department of State sent him Chamberlin’s *Russian Enigma: An Interpretation*. In this work, the veteran correspondent described the USSR as “certainly the most complete and consistent type of totalitarian state.” While cautioning that the “lusty new Russian nationalism” and the “psychology of a dictatorship” might prove complicating factors, he nonetheless judged that the prospects for postwar relations were generally good.⁵²

Three inferences were drawn from the idea of Russia redux; together, they defined what most Americans expected of their anomalous ally. First, while the USSR might use foreign communist parties as instruments of policy, it would not seek revolution for its own sake. It was, the Department of State concluded,

110 (February 21, 1944): 230–31; Edgar Snow, *The Pattern of Soviet Power* (New York, 1945), 23–25, 209–10.

⁴⁸ Freda Kirchwey, “The Red Star Rises,” *Nation*, 156 (February 27, 1943): 293–94. See also Karl Polanyi, “Why Make Russia Run Amok?” *Harper’s*, 186 (March 1943): 404–10; “What Russia Wants,” *New Republic*, 109 (October 11, 1943): 474–75; Denna Frank Flemming, *Can We Win the Peace?* (Nashville, Tenn., 1943), 105.

⁴⁹ Torbjorn Sirevag, *The Eclipse of the New Deal and the Fall of Vice President Wallace, 1944* (New York, 1985), 212–17, 247–49, 270–72, 296–305. For examples, see column by Thomas F. Woodlock, *Wall Street Journal* (January 21, 1944): 4; *Charlotte Observer* (February 13, 1944): 4.

⁵⁰ See, for example, *Chicago Tribune* (April 4, 1944): 12; *New York Herald Tribune* (December 22, 1943): 18; *New York Times* (December 22, 1943): 22.

⁵¹ Norman Thomas, *What Is Our Destiny?* (Garden City, N.Y., 1944), 79–80.

⁵² William Henry Chamberlin, *The Russian Enigma: An Interpretation* (New York, 1943), 90–91, 302–04; Elbridge Durbrow to George Elsey, January 22, 1945, RG 59, General Records of the Department of State, 861.00/1–2245.

interested in the “internal organization and politics” of neighboring states only “in so far as these affect their foreign policies.”⁵³ Second, with victory, the Soviets’ first priority would be the reconstruction of their devastated land. This implied that the United States would have a certain leverage in dealing with the USSR, for it alone could supply goods and credits for an expeditious recovery. Officials knew that the USSR would not be dependent on the United States, as even the leading advocate of economic diplomacy, Ambassador W. Averell Harriman, conceded. Their confidence derived from the conviction that communism had changed. While “the revolutionary spirit” might linger in certain quarters of the Communist party, Harriman cabled from Moscow in 1944, the “thinking of Stalin and his principal advisers” was directed “toward the consolidation and development” of their country within its frontiers of 1941.⁵⁴

Third, it was expected that the primary concern of postwar Soviet foreign policy would be security. Washington understood that the USSR would be no more willing than the United States to entrust its security to an untried international organization. From a point early in the war, officials assumed that the Soviet Union would assert “predominant interest in the whole area between Russia and Germany.”⁵⁵ That the USSR seemed no longer an agent of revolution inclined them to accept the prospect gracefully. So, too, did the supposition that postwar aid could influence Soviet behavior. The U.S. government was primarily concerned that Soviet influence in Eastern Europe should not become “so dominant as to affect international stability.” The threat to stability, officials believed, lay in the alarm that Western Europe would take if Eastern Europe became part of a Soviet sphere of *domination* and was therefore perceived as a compliant tool of potential aggression. A Soviet sphere of *influence*, on the other hand, would not be productive of discord if its purview was chiefly confined to the foreign policies of the subordinated states, as seemed probable.⁵⁶

Acceptance of such a Soviet sphere came to be seen as a means of limiting Soviet influence. In the absence of some delimitation of spheres, the developing competition between the USSR and the weaker Britain threatened to make the Soviets supreme in Europe. Late in 1944, leading Soviet specialists from the Department of State joined with their counterparts from the Office of Strategic

⁵³ “Objectives of Soviet Policy with Respect to Latin America,” box 112. See also Inter-Divisional Committee on Poland and Russia, Document CAC-71, “Soviet War Aims: A Summary View,” February 14, 1944, RG 59, Records of Harley A. Notter, box 109; remarks by Charles E. Bohlen, Chronological Minutes of the Subcommittee on Problems of European Organization, R-14 (March 3, 1944), *ibid.*, box 84. Diplomats preparing for the Moscow Conference of 1943 concluded that the Soviets would prefer a conservative government in postwar Germany since a liberal government would naturally look west, and a communist government might become a rival for the Soviet Union within the world communist movement. Policy Group, Document PG-5, “Soviet Policy on the Treatment of Germany,” September 23, 1943, *ibid.*, box 119.

⁵⁴ *Foreign Relations of the United States: Diplomatic Papers, 1945*, 9 vols. (Washington, D.C., 1967–69), 5: 938–40, 994–96.

⁵⁵ “War and Peace Aims of the Soviet Union,” box 138. See also Policy Group, Document PG-14, “Soviet Attitudes on Regional Organizations in Eastern Europe,” September 23, 1943, RG 59, Records of Harley A. Notter, box 119.

⁵⁶ Policy Committee, Document RCA-9, “Memorandum for the President: Reconstruction of Poland,” October 17, 1944, RG 59, Records of Harley A. Notter, box 137. See also Eduard Mark, “American Policy toward Eastern Europe and the Origins of the Cold War, 1941–1946: An Alternative Interpretation,” *Journal of American History*, 68 (September 1981): 313–36.

Services (OSS) to write the most comprehensive assessment of postwar Soviet foreign policy yet attempted. In order to prevent the “domination of the resources and manpower of Europe and Asia by the rising power of Russia,” the paper observed, both the United States and Britain had adopted policies of accepting the division of postwar Europe into “spheres of Soviet and non-Soviet predominance with a neutral zone [jointly occupied Germany and Austria] between” while seeking “to establish and maintain independent democratic regimes within both spheres and within the neutral zone.”⁵⁷ The agreement of October 1944, whereby Stalin and Prime Minister Winston Churchill apportioned percentages of influence in the Balkans, met with Washington’s tacit approval because it promised to curb the Anglo-Soviet rivalry for influence among Europe’s smaller states.⁵⁸

The press quickly conveyed an accurate account of Churchill’s bargain with Stalin.⁵⁹ The effect was to reinforce an already well-established expectation that postwar Eastern Europe would to some degree be subject to the Soviets. As early as January 1944, the Department of State had surveyed more than three hundred newspapers and concluded that “the bulk” were resigned to the “inevitable” prospect of “Russian dominance” in Eastern Europe.⁶⁰ The Yalta Conference (February 1945) did not change this impression. Most commentators applauded the meeting—but primarily out of relief it had convened at all. There had been fears that disputes over Poland and Greece might rend the alliance. As the Department of State noted, articulate opinion regarded the political accords affecting Eastern Europe as merely “the best obtainable” from the Soviets. The agreement on Poland is a case in point. Most editorials described it as either a capitulation to Soviet demands or as a compromise between American principles and Soviet interests.⁶¹

Opinion makers generally accepted a certain deference to the Soviets in Eastern Europe because of the importance they attached to the postwar relationship with the USSR.⁶² This position, however, did not necessarily

⁵⁷ Research and Analysis Branch, Office of Strategic Services, “Capabilities and Intentions of the USSR in the Postwar Period,” January 5, 1945, National Archives Microfilm publication M-1221, “Intelligence Reports, 1941–1961,” Report no. 2669. Participating from the Department of State were Elbridge Durbrow, Charles E. Bohlen, and Llewellyn Thompson. “USSR Division 1945,” RG 226, Records of the Office of Strategic Services, entry 1, box 23.

⁵⁸ See, for example, Policy Committee, Minutes of October 25, 1944, RG 59, Records of Harley A. Notter, box 138.

⁵⁹ *New York Times* (October 27, 1944): 1. The Department of State found that the press regarded the agreement favorably for the same reason that itself did: the averted danger of Anglo-Soviet rivalry. Department of State, “Fortnightly Survey of American Opinion on International Affairs,” no. 13 (November 6, 1944), RG 59, Records of the Office of Public Opinion Studies, box 11.

⁶⁰ Department of State, “Public Attitudes on Foreign Policy,” no. 8 (January 22, 1944); and no. 12 (February 29, 1944), RG 59, Records of the Office of Public Opinion Studies, box 1.

⁶¹ Department of State, “Fortnightly Survey of American Opinion on International Affairs,” no. 22 (March 7, 1945), RG 59, *ibid.*, box 11. The author’s survey of the press found that only 9 percent of commenting papers regarded the Yalta Accord on Poland as a fulfillment of the Atlantic Charter, while 43 percent described it as a compromise with Russian demands and 40 percent a surrender to them. Nine percent of the papers withheld judgment, pending implementation of the agreement.

⁶² See, for example, “Fortnightly Survey of American Opinion on International Affairs,” no. 8 (August 5, 1944), RG 59, *ibid.*, no. 28 (June 21, 1945); “Recent Opinion on Poland,” July 23, 1945, RG 59, *ibid.*, box 1; “Desires of the American Public in Relations with Soviet Russia,” March 28, 1946, RG 59, *ibid.*, box 45.

represent indifference to Russia's neighbors. Because of the well-established belief that communism had changed, journalists and scholars did not, any more than officials, automatically equate Soviet predominance with communization. Most predictions of the Soviet-sponsored postwar order in Eastern Europe resembled the picture that *Time* offered in 1944: "strong, popular front governments based on the trade unions, but not revolutionary and not internationalist."⁶³ The public tended to entertain an exaggerated idea of the war's effect on the Soviet Union and, consequently, of its need for postwar American aid. In a typical manifestation, Quentin Reynolds wrote in his best seller *The Curtain Rises* that peace would find the USSR "in a horribly weakened condition . . . , a giant on crutches." For that reason alone, Stalin would be "very easy to do business with" after the war.⁶⁴

Predictions and policies based on what the *New York Times* called the "nationalization of the Russian Revolution" withered amid the alarums of the cold war.⁶⁵ But their effects lingered. The memory that the United States had not resisted Soviet hegemony in Eastern Europe fueled later charges of "appeasement" once the reasons for complaisance had been forgotten or were no longer admitted. It also helped shape later perceptions of Soviet actions. The belief that the United States had "appeased" the USSR would make it difficult for Americans to see Soviet actions as stemming from anything but internal imperatives.

THE FIRST YEAR OF PEACE saw drastic changes in American perceptions of the Soviet Union. By May 1946, 60 percent of respondents to the Gallup poll believed that the Soviets wanted to dominate the world. On a trip across country that summer, journalist James Reston found Americans haunted by "a greater fear of the Russians than they ever felt for either the Germans or the Japanese." The events that persuaded most Americans that the USSR had embarked on expansionism are familiar. It suffices to observe that, by the end of 1945, few of even the Soviet Union's most adamant defenders denied that several states of Eastern Europe were being progressively subordinated to their communist parties. Using "techniques of political control . . . objectionable to democratic nations," the *New Republic* admitted in November 1945, the USSR had "exercised terrific pressures on the countries liberated by the Red Army to bring into power governments which it can manipulate."⁶⁶ The Soviet Union's defenders were

⁶³ "The Test," *Time*, 43 (January 17, 1944): 18. See also William Henry Chamberlin, "Russia after the War," *Russian Review*, 3 (Spring 1944): 3-9; "Russia's Sphere of Influence," *New Republic*, 150 (February 21, 1944): 230-31; "Stalin's Challenge to American Business," *Business Week* (December 4, 1943): 112.

⁶⁴ Quentin Reynolds, *The Curtain Rises* (New York, 1944), 43, 53.

⁶⁵ *New York Times* (January 12, 1943): 22.

⁶⁶ George H. Gallup, *The Gallup Poll: Public Opinion, 1935-1971*, 3 vols. (New York, 1972), 1: 581-82; James Reston, "The American Mood: A World Question," *New York Times Magazine* (August 4, 1946), 5 and following; "What Does Mr. Truman Mean?" *New Republic*, 113 (November 5, 1945): 587-89.

mostly liberals who sympathized with what they saw as its justifiable fears of capitalism, ever prone to crisis and war. Many more Americans, however, viewed Soviet behavior in a different context—one defined chiefly by Britain's decline and the belief that American diplomacy toward the USSR had been conciliatory to the point of compromising principle.

"I don't think anybody on this side of the ocean realized the condition that Western Europe, including Great Britain, was going to be in after the war," a prominent diplomat has recalled.⁶⁷ Before the extent of Britain's declension was evident, there was a general disposition to hold the British government at least partially responsible for Soviet behavior. Through 1945, the Department of State viewed Soviet unilateralism largely as a function of an Anglo-Soviet "power politics scramble for position" driven by "unjustified" mutual suspicions that drove "British policy farther to the right and Soviet policy farther to the left." In the fall of 1945, Secretary of State James F. Byrnes held the United States conspicuously aloof from Britain, lest the Soviets should (as he said) think that the Anglo-Americans were "ganging up" on them.⁶⁸ The Soviets were emboldened by Britain's isolation to challenge British interests in the Middle East more directly than before. Their pressure on Turkey increased in the fall of 1945, and in Iran they began openly to back northern separatists. These events coincided with a major debate in the United States over a loan to the British, during which the facts of their economic debility were widely discussed.⁶⁹

The extent of Britain's decline as a great power made it ever harder to extenuate Soviet unilateralism as part of an Anglo-Soviet cycle of reciprocal provocation. As late as March 1946, Anglo-Soviet rivalry was sometimes invoked to explain the Iran crisis of that month, in which the government of Iran complained to the United Nations about Soviet activities.⁷⁰ But, by late February, the *New York Herald Tribune* could remark that "some postulates seem pretty well accepted": the prospects for collective security had been "gravely weakened, while the alternative hope of a balance-of-power settlement had "also retreated because there is a dynamism in Soviet Russia which, coupled with the exhaustion of the old imperial powers . . . seems to dissipate any comfortable ideas of

⁶⁷ Oral history interview, John D. Hickerson, 1972, Harry S. Truman Presidential Library, Independence, Missouri.

⁶⁸ *Foreign Relations of the United States: The Conferences at Malta and Yalta, 1945* (Washington, D.C., 1955), 102–03; *Foreign Relations of the United States: Conference of Berlin, 2 vols.* (Washington, D.C., 1960), 1: 263–64. Minutes of the Committee of Three, November 6, 1945, RG 59, General Records of the Department of State.

⁶⁹ William O. McCagg, *Stalin Embattled, 1943–1948*, 212. McCagg persuasively put the developing Iran crisis in the context of a Soviet policy of playing on the imperial rivalry imputed to Britain and the United States by Lenin's theory of imperialism. See *ibid.*, 149–67. For a forthright depiction of Britain's plight by Undersecretary of State Dean Acheson, see Department of State *Bulletin*, 15 (March 31, 1945): 511–14.

⁷⁰ See, for example, *Chicago Tribune* (March 19, 1946): 10; *Boston Globe* (March 26, 1946): 11; *Kansas City Star* (March 18, 1946): 15; *Detroit News* (March 19, 1946): 18.

relaxing into a three-way status quo system.”⁷¹

The American sense of innocence vis-à-vis the USSR, a lasting legacy of wartime diplomacy, was strongly reinforced by the course that Secretary Byrnes pursued after the failure of the London Conference of Foreign Ministers (September 11–October 2, 1945). On October 31, 1945, he spoke of the USSR’s “special security interests” in Eastern Europe—the first time an American statesman had done so publicly.⁷² At the Moscow Conference (December 15–26, 1945), he recognized the manifestly undemocratic governments of Romania and Bulgaria in exchange for cosmetic changes in their cabinets. His object was to remove an obstacle to the rapid conclusion of peace treaties, the focus of his efforts through 1946. The withdrawal of Soviet forces from Eastern Europe, he reasoned, would spell the end of the region’s communist-dominated governments.⁷³ But this purpose was little understood, and his concessions seemed another instance of unreciprocated American conciliatoriness.⁷⁴

At the Moscow Conference, Byrnes had proposed a treaty to guarantee the demilitarization of Germany. His intention was to induce the Soviets to relent in Eastern Europe by removing their fears of a revanchist Germany—or to force them to show by rejection of the instrument that “security” was only a pretext for prolonging the occupation of Eastern Europe.⁷⁵ In May, before the Soviets had officially rejected the offer, news of it was made public. Nothing so discredited the USSR in the eyes of both officials and opinion makers as their indifference to the proposal. Virtually without exception, it was taken to prove “that Moscow has no real concern for its security.”⁷⁶

Earlier in the year, Byrnes had proposed international control of atomic weapons by the United Nations. While the resulting Baruch Plan had actually been carefully crafted to preserve the American nuclear monopoly for a considerable period, it won overwhelming public support as a serious effort to head off a nuclear arms race.⁷⁷ By the summer of 1946, then, the three fears

⁷¹ *New York Herald Tribune* (February 26, 1946): 26. See also Department of State, “Desires of the American Public in Relations with Soviet Russia, RG 59, Records of the Office of Public Opinion Studies, box 11.

⁷² Department of State *Bulletin*, 13 (November 4, 1945): 709–11. The dominant reaction in the press was that Byrnes had, for better or for worse, stated a policy long obvious. “Fortnightly Survey of American Opinion on International Affairs,” no. 39 (November 19, 1945), RG 59, Records of the Office of Public Opinion Studies, box 11; author’s press survey.

⁷³ U.S. Senate, Committee on Foreign Relations, *Hearings before the Committee on Foreign Relations, Eightieth Congress, First Session: Treaties of Peace with Italy, Rumania, Bulgaria, and Hungary* (Washington, D.C., 1947), 28–29.

⁷⁴ See, for example, Department of State, “Fortnightly Survey of American Opinion on International Affairs,” no. 42 (January 8, 1946), RG 59, Records of the Office of Public Opinion Studies, box 11.

⁷⁵ Diplomats were open about these purposes: *New York Times* (April 30, 1946): 1.

⁷⁶ *New Orleans Times Picayune* (May 22, 1946): 8. My own press survey found that fully 86 percent of the papers commenting on the treaty regarded it as a certain guarantee of Soviet security. President Truman pithily summarized official reaction to the Soviet’s lack of interest: Stalin’s “bluff” had been called. Diary of Joseph E. Davies, September 30, 1946, Papers of Joseph E. Davies, Library of Congress, Washington, D.C.

⁷⁷ Gregg Herken, *The Winning Weapon: The Atomic Bomb in the Cold War, 1945–1950* (New York, 1981), 171–91. Even many liberals usually favorable to the USSR were dismayed by the Soviet rejection: Hamby, *Beyond the New Deal*, 106.

most commonly invoked to exculpate Soviet unilateralism—Britain, Germany, and the atomic bomb—had ceased to be widely credible. And the United States itself was not in the eyes of most of its citizens a candidate to become a fourth.

In recent years, there has been a considerable illumination of the geopolitical vision that informed policy making and the commanding heights of public opinion during the early cold war. Reduced to essentials, it was balance-of-power politics writ large for the air age. The new doctrine held that the United States had to prevent Eurasia's falling to any single power, for an imperium that united the population of Asia with the workshops of Western Europe and Japan would be dangerously stronger than an America no longer protected by its girding seas. This plausibly explains why Soviet expansionism, as highlighted by Britain's decline and American diplomacy, seemed threatening. What it does not explain is the radical reinterpretation of Soviet motives during 1946.⁷⁸

The national-interests model of Soviet foreign policy was not inconsistent with expansion on a grand scale. There had long been speculation that a postrevolutionary Russia might, like France before it, enter a Napoleonic phase. Early in 1944, President Roosevelt had publicly warned that "the trend from Marxism to nationalism in Russia" might portend an expansionism "no less disquieting than attempts at world revolution."⁷⁹ Given the reigning geopolitical concepts, the national-interests model was quite compatible with perceptions of a Soviet threat to American security interests. Soviet "hegemony over the Eurasian continent," the *Saturday Evening Post* had remarked in 1944, would be alarming "if Russia were ruled by Ivan the Terrible or a middle class regime as 'capitalistic' as General Electric."⁸⁰

Events in Eastern Europe were, of course, important in transforming perceptions of Soviet motives. But they must be kept in perspective. American opinion about the USSR, both public and official, had decisively changed by the summer of 1946, although the evidence from Eastern Europe was still mixed at that time. The Soviets tolerated representative governments in Hungary, Czechoslovakia, Finland, and jointly occupied Austria. Communist parties did dominate the coalition governments of Poland, Bulgaria, and Romania—but denied that Soviet-style socialism was their objective. It might be argued that suspicions of the Soviets were such that even the far-from-complete communization of these countries sufficed to "prove" the worst. This was doubtless the case for many people. But it is instructive to note that the blatant Soviet aggressions of 1939–1940 had *strengthened* the national-interests model of the United States' Soviet foreign policy, even though the attack on Finland had inspired an

⁷⁸ See, for example, John Lewis Gaddis, *The Long Peace: Inquiries into the Origins of the Cold War* (New York, 1987), 22–26; Melvyn Leffer, "The American Conception of National Security and the Beginnings of the Cold War, 1945–48," *AHR*, 89 (April 1984): 356–59.

⁷⁹ Forrest Davis, "What Really Happened at Teheran," *Saturday Evening Post*, 215 (May 13, 1944): 12–13. This was a thinly disguised interview with Roosevelt, who reviewed the manuscript before publication and subsequently commended Davis for accurately stating his views. Forrest Davis to Steven Early, March 23, 1944, Papers of Franklin D. Roosevelt, Official File, folder 4287, Franklin D. Roosevelt Presidential Library, New York; Roosevelt to Davis, June 14, 1944, *ibid.* See also *New York Times* (December 22, 1943): 22; Chamberlin, *Russian Enigma*, 191.

⁸⁰ "Stalin Can't Ignore Storm Warnings," *Saturday Evening Post*, 216 (November 11, 1944): 108.

outburst of anger that had no equal in the early postwar period.⁸¹ The Soviet actions of 1939–1940, moreover, had closely conformed to the National Bolshevik Paradigm’s model of revolutionary imperialism. Stalin’s armies had installed on Finnish soil a communist government headed by the veteran Comintern operative, Otto Kuusinen. The Balticum, eastern Poland, and the provinces wrenched from Romania were quickly and ruthlessly sovietized. Postwar Soviet behavior was by comparison circumspect.

Despite his private outrage at the attack on Finland, President Roosevelt said almost nothing publicly because he suspected that Stalin’s alliance with Hitler was fragile. In 1946, by contrast, the Truman administration worked to prepare public opinion for a firmer policy toward the USSR. But there were as yet distinct limits to what officials were willing to say about a power they still described as an ally. Byrnes was chiefly responsible for the process of reeducation. He gave radio addresses throughout the year in which he repeatedly cataloged Soviet obstacles to the conclusion of peace treaties. But he said nothing to suggest that a reconsideration of the Soviet government’s motives was in order, nor did he even directly accuse it of expansionism.⁸² President Truman carefully abstained from any public criticism of the Soviets throughout 1946.⁸³ And yet, without overt official encouragement, 70 percent of the public had concluded by October that the USSR plotted global domination.⁸⁴

There are, however, two differences between the first and second periods of Soviet expansion that largely explain the differing American reactions to them. Hitler’s defeat, first of all, had removed the external justification for defensive expansion that the USSR had had in 1939–1940. Postwar domestic developments in the USSR constituted the second difference. During the first period of Soviet expansionism, Soviet domestic policy had continued to reinforce the image of Stalinist “conservatism.”⁸⁵ Americans therefore tended to assign “conservative” motives—security or nationalistic imperialism—to Stalin’s behavior. Postwar Soviet expansion, on the other hand, was accompanied by an ideological reconversion that forced a reconsideration of what most Americans had come to believe about Soviet Communism over nearly two decades.

There had been adumbrations of the change in the Soviet domestic line as early as the winter of 1944–1945. Lenin’s theory of imperialism, tactfully shelved in 1941, reappeared in Soviet theoretical journals, specifically applied to the

⁸¹ Robert Sobel, *The Origins of Interventionism: The United States and the Russo-Finnish War* (New York, 1960), 109–17.

⁸² Even in his address before the Overseas Press Club on February 28, 1946, commonly regarded as a turning point in American foreign policy, Byrnes spoke of “our soviet Ally,” adding that “only an inexcusable tragedy of errors could cause serious conflict between us in the future”; Department of State *Bulletin*, 14 (March 10, 1946): 358. See also *ibid.*, March 24, 1946: 481–83; June 2, 1946: 950–54; 15 (July 28, 1946): 167–72; August 4, 1946: 202; August 11, 1946: 251–52; August 18, 1946: 313–19; August 25, 1946: 352–54; October 27, 1946: 739–43.

⁸³ At his press conferences, for example, Truman always deflected or defused sharp questions about the USSR. See, for example, *Public Papers of the Presidents of the United States: Harry S. Truman, 1946*, 8 vols. (Washington, D.C., 1961–66), 146, 155–56, 211, 243, 316.

⁸⁴ Department of State, “U.S. Opinion of Russia,” October 31, 1946, RG 59, Records of the Office of Public Opinion Studies, box 45.

⁸⁵ In particular, the imposition of a harsh labor code and the ending of free education. See, for example, Bertram D. Wolfe, “The Silent Soviet Revolution,” *Harper’s*, 183 (June 1941): 10–18.

United States and Britain. It implied that the two leading imperialist powers would be plagued by postwar crises of overproduction and would fall to contending for markets. They might well go to war; they might also, however, find a common cause and attack the Soviet Union in search of a respite from their problems. The warning found a low-key official endorsement in August 1945, when President Mikhail Kalinin told a delegation of collective farmers that the USSR still lived in the "capitalist encirclement" implied by Lenin's theory.⁸⁶

Even American diplomats in Moscow scarcely noticed these omens. Stalin's authoritative reaffirmation of the Leninist theory of imperialism in his famous election speech of February 9, 1946, was, accordingly, a great surprise. The address was understated and devoted primarily to announcing a new Five-Year Plan. It was left to Stalin's colleagues to develop the dangers of the "capitalist encirclement" in companion speeches.⁸⁷ American officials, largely because of George F. Kennan's famous "Long Telegram" of February 22, tended to regard the election speech as a renunciation of alliance.⁸⁸ But there was no immediate revolution in public opinion. Some commentators were alarmed, but others saw in the announcement of the Five-Year Plan proof of "socialism in one country," and in the evocation of encirclement "just campaign oratory" to spur an exhausted people to work for the recovery of their country.⁸⁹

But there followed what the Department of State described in May as "a general return to the ideology and principles of the period before 1935." It hit full stride that summer with the beginning of the notorious *Zhdanovshchina*, the program of ideological purification led by Stalin's heir apparent, Andrei Zhdanov. Of particular concern to American observers were the repeal of concessions made earlier to religion and the displacement of Russian nationalism by a Soviet nationalism that stressed the multinational character of the USSR and its revolutionary duties abroad.⁹⁰ In dealing with foreign diplomats and journalists, Stalin continued to be a model of conciliation. In October, he denied not only the existence of a "capitalist encirclement" but the very possibility of one.⁹¹ But the contrast between these words and the strident line fed daily to the Soviet people was too marked for such reassurances to have any lasting effect on observers long used to gathering clues from the Soviet domestic scene.⁹²

⁸⁶ McCagg, *Stalin Embattled*, 156–60. There are various explanations of what caused the postwar turn toward a hard line. For a brief overview of the scholarship, see Jack Snyder, "The Gorbachev Revolution: A Waning of Soviet Expansionism?" *International Security*, 12 (Winter 1987–88), 93–103.

⁸⁷ McCagg, *Stalin Embattled*, 159–60. For the most complete summary of the addresses in English, see Department of State, Office of Research and Analysis, "The Election Speeches of the Soviet Leaders," March 4, 1946, National Archives Microfilm Publication no. 1221, "Intelligence Reports, 1941–1961," Report no. 3554.

⁸⁸ For the impact of Kennan's cable, see Yergin, *Shattered Peace*, 170–71.

⁸⁹ See, for example, *Baltimore Sun* (February 10, 1946): 12; *Christian Science Monitor* (February 12, 1946): 14; *New York Herald Tribune* (February 11, 1946): 20; *Omaha World Herald* (February 12, 1946): 6; *Washington Post* (February 16, 1946): 6.

⁹⁰ Department of State, "Union of Soviet Socialist Republics: Policy and Information Statement," May 15, 1945, Papers of Clark M. Clifford, box 15, Truman Library; Frederick L. Barghoorn, *Soviet Russian Nationalism* (New York, 1956), 234–45, 258–60; McCagg, *Stalin Embattled*, 250–54.

⁹¹ *New York Times* (October 29, 1946): 1.

⁹² See, for example, *Foreign Relations of the United States: Diplomatic Papers, 1946*, 11 vols. (Washington, D.C., 1969–1972), 6: 794–96; *Boston Globe* (November 23, 1946): 8; *Louisville Courier-Journal* (September 25, 1946): 6; *Charlotte Observer* (September 25, 1946): 5.

Since all the interpretations of Stalinism were at root efforts to explain the apparent contradictions between Soviet praxis and Marxist theory, resurgent orthodoxy could only call into question what expert and ordinary people alike had come to believe about Soviet Communism. The Thermidorian Paradigm, little more than a projection of conservative pieties, was immediately vulnerable to a convincing demonstration that the “unnatural” ideology of Marxism still flourished in the Soviet Union. After the election speeches, the *New York Times* lamented that the entire basis of American policy toward the USSR—“the Soviet Government’s explicit renunciation of any aims of world revolution” and the development “of Russian patriotism and nationalism”—had been rudely thrown into doubt.⁹³

Many conservatives quickly concluded that Stalin’s moderation had been a cunning expedient and gravitated toward the National Bolshevik Paradigm. Its stress on communism’s tactical flexibility reconciled the new party line with the surviving manifestation of the Thermidor that had for so long fascinated the Right. Stalin, a leading Republican explained in mid-1946, had revived piece-work and promoted family life to strengthen the Soviet Union. That these steps were “anathema to American Communists” should not obscure the fact that the dictator remained a “firm believer in his creed.” Soviet “nationalism” now appeared in a new light. With the opportunity, the *Times* concluded, “the masters of the Kremlin had returned to their first hate, the ‘capitalist states,’ and have declared against them a political war in which Russian imperialism is blended with the communist doctrine of world revolution.”⁹⁴

Closely coinciding with the reevaluation of Soviet Communism, a wave of strikes gripped the United States. It began late in 1945 and lasted through 1946. In the fall of the latter year, business counterattacked with a campaign coordinated by the U.S. Chamber of Commerce that featured charges of both communist influence in unions and of Soviet control over the communists. During the elections of 1946, Republicans repeated their old charges of communism in government.⁹⁵ Red-baiting political tactics did not require a threatening portrayal of the Soviet Union. Conservatives, after all, had a well-established tradition of attacking domestic radicals while claiming Stalin as

⁹³ *New York Times* (February 11, 1946): 28. See also “Getting Tough with Russia,” *Life*, 20 (March 18, 1946): 36; *Cleveland Plain Dealer* (February 11, 1946): 2; *Chicago Daily News* (February 12, 1946): 8.

⁹⁴ Karl Mundt, “Can We Get Along with Russia?” *Vital Speeches*, 12 (June 15, 1946): 514–22; *New York Times* (November 20, 1947): 28. The National Bolshevik Paradigm also explained the distinctive character of the Soviet sphere. Had Stalin been merely an “old-style imperialist,” wrote a leading student of Soviet foreign policy, he would have been content to control Eastern Europe “whatever the political or economic system might be.” But, as he was a “world revolutionist,” his imperialism was of “the revolutionary variety.” David J. Dallin, “The Consistency of Stalin,” *American Mercury*, 62 (January 1946): 7–15; See also “Red Star Rising,” *Fortune*, 34 (July 1946): 106–07 and following; William C. Bullitt, *The Great Globe Itself: A Preface to World Affairs* (New York, 1946), 92–109; *Los Angeles Times* (May 21, 1946): II, 4; *Foreign Relations of the United States: Diplomatic Papers, 1946*, 5: 795–97.

⁹⁵ Peter H. Irons, “American Business and the Origins of McCarthyism: The Cold War Crusade of the United States Chamber of Commerce,” in Robert Griffith and Athan Theoharis, eds., *The Specter: Original Essays on the Cold War and the Origins of McCarthyism* (New York, 1974), 74–89; James Boylan, *The New Deal Coalition and the Election of 1946* (New York, 1981), 135–38.

something like a kindred soul.⁹⁶ But, when the rapid eclipse of the Thermidorian Paradigm had removed this option, business and the Republican party put the National Bolshevik Paradigm to use. American Communists were too few to be credible as a revolutionary force, as even their most devoted antagonists sometimes conceded. But they could be plausibly portrayed as the fifth column of a foreign power that embodied their ideology. It was on that ground that legal sanctions against the Communist party were sought and ultimately obtained.⁹⁷ But this was an effect, not a cause, of the earlier, generalized change in perceptions of the USSR that made it plausible, and therefore profitable, to invoke the specter of Soviet-directed world revolution.

Unlike the National Bolshevik Paradigm, the Totalitarian could accommodate the Soviet Union's ideological reconversion. Socio-political in emphasis, it stressed the interests of the dictatorship and its servitors of the "new class," for which Marxism-Leninism had become a rationalization.⁹⁸ Since the Soviet state was the vehicle for those interests, the Totalitarian Paradigm had stressed the nationalism of the USSR's foreign policy. But there had been a suggestion of danger. The "capitalist encirclement" had been first invoked before fascism threatened the USSR.⁹⁹ It had coexisted easily enough with the conciliatory diplomacy of the 1920s. But the formation of the Stalinist system, with all its extremes of terror and privilege, had mostly followed Hitler's rise to power. Full-blown Stalinism, then, had never existed without a strong external justification. During the war, this circumstance had suggested to some that, with the destruction of the Axis, the Soviet elite would have to find new enemies to justify its tyranny and the indefinite deferral of the utopian promises of Marxism.¹⁰⁰

Proponents of the Totalitarian Paradigm, while perhaps surprised by the resurrection of the "capitalist encirclement," were not theoretically discomfited. From the mid-1930s, George F. Kennan had stressed the totalitarian character

⁹⁶ Even for the U.S. Chamber of Commerce, the internal struggle against the Communist party and Soviet-American relations were separable issues. It signaled the beginning of the former with an article in its publication, *Nation's Business*, that warned about communist penetration of unions. The same issue, however, contained a favorable review of Soviet foreign policy. Carlisle Bergeron, "Our Communists Reconvert," *Nation's Business*, 33 (September 1945): 80 and following; Charles Prince, "Ten Things Russia Wants," *ibid.*, 28–29 and following. The articles in *Nation's Business* about the USSR were either neutral or favorable through May 1946.

⁹⁷ Peter Steinberg, *The Great "Red Menace": United States Persecution of American Communists, 1941–1952* (Westport, Conn., 1984), 60–61.

⁹⁸ See, for example, Alexander Kirk to Cordell Hull, April 13, 1939, RG 59, General Records of the Department of State, 861.00—PARTY ALL UNION COMMUNIST/221; *Foreign Relations of the United States: The Soviet Union, 1933–1939*, 520–21; Michael T. Florinsky, *Toward an Understanding of the USSR* (New York, 1939), 223–27; Peter Drucker, *The End of Economic Man* (New York, 1939), 195–99, 244–47.

⁹⁹ The USSR, in fact, was economically dependent on the very states said to be mounting an anti-Bolshevik crusade, and the Red Army was largely armed with weapons purchased from them. Adam Ulam, *Expansion and Coexistence: The History of Soviet Foreign Policy, 1917–1967* (New York, 1968), 165–66; Mikhail Heller and Aleksandr M. Nekrich, *Utopia in Power: The History of the Soviet Union from 1917 to the Present* (New York, 1986), 215.

¹⁰⁰ See, for example, Stephen P. Duggan, *Professor at Large* (New York, 1943), 225; book review by John Chamberlain, *New York Times* (November 2, 1943): 23; remarks by Charles E. Bohlen, Minutes of the Subcommittee on Problems of European Organization of the Advisory Commission on Postwar Foreign Policy, R-14, March 3, 1944, RG 59, Records of Harley A. Notter, box 84; "What Russia Needs Is Honest News," *Saturday Evening Post*, 218 (November 10, 1945): 132.

of the Soviet regime.¹⁰¹ "The fire of revolutionary Marxism has definitely died out," he wrote in 1945. It was "questionable" that it animated "to any appreciable degree the power of the Kremlin." Before February 1946, he described Soviet policy as not dissimilar to that of the tsars. Thereafter, he emphasized the domestic imperatives of totalitarian rule. A "hostile international environment," he wrote in March, 1946, was the "breath of life" for the Soviet regime, without which there could be "no justification for the tremendous and crushing bureaucracy of party, police and state which now lives off the labor and idealism of the Russian people."¹⁰² Similar explanations came both from conservatives like Walter Lippmann and representatives of the anticommunist Left like Reinhold Niebuhr, who had long stressed the class-oriented nature of Soviet politics.¹⁰³ Innate, unappeasable, and ever in need of "enemies," the insecurity of the "new class" was functionally equivalent to Bolshevism's dreams of world revolution.¹⁰⁴

When President Truman stepped before Congress on March 12, 1947, to request emergency aid for Greece and Turkey, he cited the threat of "totalitarianism." Many officials, Truman among them, believed in the Totalitarian Paradigm.¹⁰⁵ But there were also sound political reasons why totalitarianism was the officially designated enemy of the early cold war. The National Bolshevik Paradigm was associated with the Right. The Totalitarian Paradigm, while appealing to many conservatives, was not offensive to the anticommunist Left, which had adopted it a decade before.¹⁰⁶ It was therefore suited for building a consensus behind the developing policy of containment. In Europe, socialists such as Clement Atlee, Ernest Bevin, Kurt Schumacher, Leon Blum, and Karl Renner were the most effective anticommunists. To avoid offense to them, or help to shortsighted conservative congressmen who wished to use American aid to fight socialism as well as communism, it was politic to distance the Soviet

¹⁰¹ In 1932, Kennan wrote that the USSR was "still being administered in the interests of a doctrine." By 1937, he had concluded that Stalin's only philosophy was "L'Etat—c'est moi!" Robert F. Skinner to Henry L. Stimson, August 19, 1932, enclosing memorandum by George F. Kennan, RG 59, General Records of the Department of State, 861.00/11496; Loy W. Henderson to Cordell Hull, May 26, 1937, *ibid.*, 861.20/396 (Kennan's draft).

¹⁰² George F. Kennan, *Memoirs (1925–1950)* (Boston, 1967), 537, 541; *Foreign Relations of the United States: Diplomatic Papers, 1946*, 6, 721–23.

¹⁰³ Column by Walter Lippmann, *Washington Post* (May 5, 1947): 5; Reinhold Niebuhr, "The Fight for Germany," *Life*, 11 (October 21, 1946): 65–66 and following; Reinhold Niebuhr, "Russia and Karl Marx," *Nation*, 145 (May 7, 1938): 530–31. See also James Marshall, "Curtains of Iron and Nylon," *Saturday Review of Literature*, 29 (June 29, 1946): 5 and following; column by Drew Pearson, *Washington Post* (August 2, 1946): 4; "The Wallace Speech," *Commonweal*, 44 (September 1946): 563–64; *Boston Herald* (March 17, 1946): 22; *Baltimore Sun* (February 10, 1946): 12.

¹⁰⁴ Thus Kennan told one audience in 1947 that the Soviet goal was "world revolution," adding the easily overlooked qualification that the early Bolshevik objective had been retained because "for reasons too intricate to go into here, it has become closely associated with internal political conditions in the Soviet Union; and the men in the Kremlin could not depart from it, even if they wished to"; George F. Kennan, "Current Problems of Soviet-American Relations," May 9, 1947, Papers of George F. Kennan, Princeton University Library, Princeton, New Jersey.

¹⁰⁵ "There is no socialism in Russia," President Truman wrote in his diary in 1945. "It is the hotbed of special privilege" and "just police government pure and simple"; William Hillman, *Mr. President* (New York, 1952), 121.

¹⁰⁶ For examples of conservative and leftist use of the Totalitarian Paradigm, see, respectively, Joseph R. Evans, "Europe's Political War," *Wall Street Journal* (March 10, 1947): 4; and Dwight Macdonald, "The Root Is Man," *Politics*, 3 (April 1947): 97–115.

Union as much as possible from socialism.¹⁰⁷ Thus the deeply conservative Kennan somewhat incongruously bade Americans to “distinguish what is indeed progressive social doctrine from the rivalry of a foreign political machine which has appropriated and abused the slogans of socialism.”¹⁰⁸

The Evolutionary Paradigm, like the Thermidorian, was vulnerable to the charge that it had failed the test of prediction. It assumed that “socialism in one country” was still current doctrine. “Communists everywhere,” Henry Wallace said in explanation of his opposition to the Truman Doctrine, “want, eventually, a Communist world.” But, he continued, “they are essentially interested, at the moment, in strengthening the Soviet Union as an example of the type of socialism they have in mind.”¹⁰⁹ Otherwise, the Evolutionary Paradigm differed little from the National Bolshevik: both credited Stalin’s Marxism. Not surprisingly, the fate of Eastern Europe persuaded some writers long associated with the Evolutionary Paradigm, among them the prominent liberal George S. Counts, to abandon it for the National Bolshevik.¹¹⁰

Most liberals were not so easily persuaded.¹¹¹ They saw Eastern Europe and the “capitalist encirclement” in a distinctive context. The Leninist theory of imperialism, as invoked in Stalin’s election speech, bore more than a passing resemblance to the fears about America that they had harbored even since the conservative recrudescence of 1942. “We accept Premier Stalin’s denunciation of monopoly capitalism,” the *New Republic* declared in February 1946. With “unanswerable logic,” the Soviet ruler had shown “that stability in capitalist countries is a precondition of peace.”¹¹² Anglo-American support for conservative groups in Europe and elsewhere, a concern of liberals during the war, had grown and seriously alienated those who still cherished a vision of the global awakening of the “Common Man.” Liberal critics had predicted that if, out of fear of communism, the Western Allies adopted a policy in support of conserv-

¹⁰⁷ The Department of State had been greatly concerned to align the United States with the democratic Left in Europe as early as 1944. See minutes of the Policy Committee, May 1, 4, 11, and 15, 1944, RG 59, Records of Harley A. Notter, box 137. For a representative explanation of the importance of socialists for American policy in Europe, see Herbert L. Mathews, “Socialism vs. Communism—The Stake,” *New York Times Magazine* (February 17, 1946): 10 and following.

¹⁰⁸ Kennan, *Memoirs (1925–1950)*, 301.

¹⁰⁹ *New York Times* (July 6, 1947): 27. See also Harrison Salisbury, *Russia on the Way* (New York, 1946), 352–58; Edgar Snow, *Stalin Must Have Peace* (New York, 1946), 116–26; Ralph Barton Perry, “The Logic of Peace,” *Atlantic Monthly*, 180 (December 1947): 76–85.

¹¹⁰ George S. Counts, “Russia Softens History,” *Christian Science Monitor Magazine* (October 6, 1946): 5. Compare George S. Counts and John L. Childs, *America, Russia, and the Communist Party in the Postwar World* (New York, 1943), 18, 45–88. Another prominent exponent of the Evolutionary Paradigm, in the course of a plea for an understanding of the USSR, posed these as the ultimate alternatives for Soviet Russia: “Either develop her Communist state internally and convince the world by propaganda and example to follow her lead; or launch upon the conquest of the world”; Ely Culbertson, *Must We Fight Russia?* (Philadelphia, 1946), 13–14.

¹¹¹ This statement is based on the greater popularity among liberals through 1947 of the Progressive Citizens of America over Americans for Democratic Action, apparently because of the latter’s support for the foreign policy of the Truman administration. Mary Sperling McAuliffe, *Crisis on the Left: Cold War Politics and American Liberals* (Amherst, Mass., 1978), 23–24.

¹¹² “Stalin’s Speech,” *New Republic*, 114 (February 18, 1946): 235–36. It was “a certainty,” a regular contributor to the *New Republic* had written a few months earlier, that a depression would bring “would-be American Hitlers” who would seek a solution to domestic problems through war with the USSR. William Mandel, “Getting Along with Russia,” *ibid.*, 113 (August 27, 1945): 245–46.

ative regimes after the war, the Soviets would cultivate clients of their own. In essence, their understanding of the early cold war was that the great powers, gripped by mutual suspicions for which the West bore chief responsibility, were engaged in a race for strategic advantage that threatened another war.¹¹³

The Soviet Union's guilt was further mitigated by its choice of clients. While those of the United States and Britain were conservative elites, the USSR, as a socialist state, made mutually beneficial alliances with the discontented masses. While it was "safe to be cynical" about Soviet policy, Freda Kirchwey observed, "one of the complexities often overlooked is the interweaving in Soviet diplomacy of tough self-interest and honest support of reforms that mean more to a people robbed of the primitive decencies of life than even the best American-style election."¹¹⁴ This defense failed for several reasons. It did not accord with the sense of innocence that most Americans had about their country's relations with the USSR, and it rested on an interpretation of Stalinism that (like the Thermidorian Paradigm) was becoming increasingly implausible. Its premises about the dangers of "monopoly capitalism" and the greater urgency of "economic" than "political" democracy, moreover, were decidedly sectarian in their appeal.

TO GRASP THE EVOLUTION OF AMERICAN THOUGHT about the nature and purposes of Soviet Communism from the 1930s through the early cold war, it is important to realize that Americans were acutely conscious of Stalin's departures from the practices of early Bolshevism. Many of his apostasies were front-page news in the decade of the depression. Curiosity at the outcome of one of the transforming events of the age was inevitable, but it was sharpened by the symbolic significance of the unfolding drama for American politics. Every shade of political opinion had a stake in the explication of the "Soviet Experiment." It was, then, not enough that an explanation of Stalinism should be sufficiently plausible to satisfy the natural curiosity of American onlookers; the Thermidorian, Evolutionary, and Totalitarian paradigms triumphed in the 1930s because they affirmed the values of important constituencies.

Historians have often discussed the problem that Stalinism posed for the Left. But that it represented an intellectual challenge for other Americans, too, has been lost to memory. It is for that reason that historians of Soviet-American relations have so dismissively attributed the wartime belief that Stalin was a

¹¹³ See, for example, Frederick L. Schuman, "A Diagnosis of the Big Three Problem," *New York Times Magazine* (June 30, 1946): 6 and following; I. F. Stone, "The U.S. and the U.S.S.R.," *Nation*, 163 (October 26, 1946): 460–61; Vera Micheles Dean, "Russia and Her Neighbors in Europe," *Atlantic Monthly*, 177 (March 1946): 69–74; Michael Straight, "Fixing the Blame for the Cold War," *New Republic*, 117 (September 15, 1947): 10–15.

¹¹⁴ Freda Kirchwey, "Old Game, New Rules," *Nation*, 161 (December 29, 1945): 725–26. See also Joachim Joesten, "Silent Revolution in Eastern Europe," *Virginia Quarterly Review*, 22 (Spring 1946): 213–23; Jane Bedell, "Border Trouble," *New Republic*, 116 (February 3, 1947): 460–61; Stone, "The U.S. and the U.S.S.R."; Dean, "Russia and Her Neighbors in Europe."

nationalist, even a conservative, to mere wishful thinking.¹¹⁵ There has been a greater awareness of the political uses to which Soviet Communism was put. But they figure in histories of the cold war only as “anti-Communism,” construed primarily as conservative antiradicalism. But liberals, too, used their version of Stalinism, first to defend the possibility of social change, then to counter what they perceived as America’s fascist tendencies. Even “anti-Communism” was more complex in its origins and diverse in its effects than has been allowed. It was, first of all, a tendency of the Left as well as of the Right, rooted not only in opposition to socialism but also in anguish at Stalin’s perceived betrayal of it. Nor was “anti-Communism” simply a cause of discord between the United States and the Soviet Union. It was a force for harmony to the extent that it led many radicals and conservatives to minimize Stalin’s commitment to socialism—the radicals because they wished to show that socialism had never been tried, the conservatives because they wanted a demonstration of its impracticability. Strange as it may seem, the sanguine views that so many Americans held of their strange ally during World War II were largely based on “anti-Communism.”¹¹⁶

The tests of plausibility and political appeal still obtained in the postwar period. Conservatives did not suddenly lose their interest in discrediting the possibility of sweeping social change by pointing to a Stalinist Thermidor; the Soviet ideological reconversion discredited the argument. Just as the Thermidorian Paradigm failed to account for postwar Soviet domestic policy, so the Evolutionary, based on “socialism in one country,” could not explain Soviet foreign policy without propositions about “monopoly capitalism” and American foreign policy that most Americans could not, or would not, believe. The National Bolshevik and Totalitarian paradigms were left in possession of the field, partially by default but also because each in its own way reconciled the surviving manifestations of Stalinist “conservatism” with renewed ideological militancy at home and expansion abroad. The former also served a purpose of the Right—antiradicalism—and the latter a purpose of the Left—the redemption of collectivism. In concert, they facilitated the formation of a national consensus in support of containment.

¹¹⁵ One reason for this, it would appear, is that students of Soviet Communism, on whom diplomatic historians must ultimately rely for most of their knowledge about the USSR, state with a curious confidence that, before the current generation of scholars, no one in the United States paid any attention to Stalin’s departures from Bolshevism, save for Trotskyists and a handful of unusually perceptive scholars. See, for example, Stephen F. Cohen, *Rethinking the Soviet Experience: Politics and History since 1917* (New York, 1985), 40–41; Robert V. Daniels, *Russia: The Roots of Confrontation* (Cambridge, 1985), 175–76.

¹¹⁶ As the philosopher Ralph Barton Perry observed in 1942. Perry to the *New York Times* (July 12, 1943): IV, 8.

The Age of Vulnerability: Eisenhower and the National Insecurity State

H. W. BRANDS

ALTHOUGH 1945 TRADITIONALLY MARKS THE BEGINNING of the nuclear era in international affairs, not until more than a decade later did the United States and the Soviet Union possess sufficient numbers of sufficiently powerful and deliverable weapons to threaten each other's existence. The impact of the approach of Armageddon on the leadership of Soviet society remains largely a matter of conjecture; despite *glasnost*, the keepers of the Kremlin's secrets appear unlikely in the near future to lay open their records on nuclear and national-security planning during the 1950s. While access to American documents is not what it should be, enough material has become available during the past few years to allow a relatively thorough reconstruction of the manner in which American policy makers, most notably in the administration of Dwight D. Eisenhower, tackled the problems that have vexed American national-security planning ever since: how to integrate nuclear weapons into overall defense strategy, how to defend the United States against an opponent armed with such weapons, and how to balance economic health against military strength.¹

The documents tell a story with two themes. The first, having implications primarily for the historiography of the period, involves the process of policy making during the 1950s. The Eisenhower to whom we have recently been reintroduced—the masterful chief executive, knowledgeable about the issues and firmly in command of his administration—makes occasional appearances here; but equally often we see a president slow to realize the import of the revolutionary changes confronting America and unable to control the bureaucracy that made policy in his name. The result was administrative confusion, at times approaching paralysis.²

¹ The development of Soviet national-security policy is not entirely a blank page; for a recent guide to the literature, see William C. Green, *Soviet Nuclear Weapons Policy: A Research and Bibliographic Guide* (Boulder, Colo., 1987).

² Representative works of the last decade or so on the Eisenhower administration include Charles C. Alexander, *Holding the Line: The Eisenhower Era, 1952–1961* (Bloomington, Ind., 1975); Elmo R. Richardson, *The Presidency of Dwight D. Eisenhower* (Lawrence, Kans., 1979); Richard H. Immerman, "Eisenhower and Dulles: Who Made the Decisions?" *Political Psychology*, 1 (Autumn 1979): 3–20; Robert A. Divine, *Eisenhower and the Cold War* (New York, 1981); Blanche Wiesen Cook, *The Declassified Eisenhower: A Divided Legacy* (Garden City, N.Y., 1981); William B. Ewald, Jr., *Eisenhower the President: Crucial Days, 1951–1960* (Englewood Cliffs, N.J., 1981); Fred I. Greenstein, *The Hidden-Hand Presidency: Eisenhower as Leader* (New York, 1982); Robert Griffith, "Dwight D. Eisenhower and the Corporate Commonwealth," *AHR*, 87 (February 1982): 87–122; Stephen E.

The second theme treats larger matters and goes to the heart of the American national—and individual—experience in the nuclear age. Before the 1950s, the countries of the world knew danger; neither the powerful, such as Britain, nor the isolated, like the United States, felt completely immune from enemy threats. But only the weakest countries faced the possibility of national extinction. For the rest, hostile action might entail defeat and surrender; yet life would go on. During Eisenhower's tenure as president, such assurance vanished. Even Americans, inhabitants of the mightiest nation on earth, found themselves alarmingly vulnerable. They still exercised considerable control over their destiny, but, whatever measures Washington might take, the safety of Americans now depended in large part on the good behavior of the country they had identified as their mortal enemy. For those charged with guaranteeing the national security—for Eisenhower and his associates—the problem proved overwhelming and, ultimately, insoluble. Their efforts to remedy the situation failed, and they were forced to the conclusion that national security no longer existed. The best they could hope for was a policy that would minimize national insecurity. Unfortunately, and portentously, they did not achieve even this.³

Ambrose, *Eisenhower*, vol. 2: *The President* (New York, 1984); Piers Brendon, *Ike: His Life and Times* (New York, 1986); Richard A. Melanson and David Mayers, eds., *Reevaluating Eisenhower: American Foreign Policy in the 1950s* (Urbana, Ill., 1987); and H. W. Brands, Jr., *Cold Warriors: Eisenhower's Generation and American Foreign Policy* (New York, 1988).

³ Four recent studies of Eisenhower and national-security management are particularly useful. Douglas Kinnard, *President Eisenhower and Strategy Management: A Study in Defense Politics* (Lexington, Ky., 1977), portrays Eisenhower as "a skilled practitioner" in matters of defense policy (123, 135–36). Ambrose, *Eisenhower*, is also laudatory. He praised Eisenhower for restraining defense spending and the pace of the arms race (2: 625–26). David Alan Rosenberg, "The Origins of Overkill: Nuclear Weapons and American Strategy," in Norman A. Graebner, ed., *The National Security: Its Theory and Practice, 1945–1960* (New York, 1986), 123–98, offers a different view. Despite Eisenhower's knowledge of nuclear weapons and his ability to formulate general policy guidelines, Rosenberg argued that he too often failed to follow through on matters of detail (141). Although John Lewis Gaddis, *The Long Peace: Inquiries into the History of the Cold War* (New York, 1987), also noted a "distinction between declaratory and actual policy," he saw this not as the result of bureaucratic inertia or intrigue but as deliberate obfuscation—a matter of Eisenhower talking tougher than he acted (144–45). The present study finds more to criticize in Eisenhower's performance than any of these, although it is closer to Rosenberg than to the rest.

Other important works include Thomas C. Schelling, *The Strategy of Conflict* (Cambridge, Mass., 1960); Samuel P. Huntington, *The Common Defense: Strategic Programs in National Politics* (New York, 1961); Thomas C. Schelling and Morton H. Halperin, with Donald G. Brennan, *Strategy and Arms Control* (New York, 1961); Glenn Herald Snyder, *Deterrence and Defense: Toward a Theory of National Security* (Princeton, N.J., 1961); Warner R. Schilling, Paul Y. Hammond, and Glenn H. Snyder, *Strategy, Politics, and Defense Budgets* (New York, 1962); Alexander George and Richard Smoke, *Deterrence in American Foreign Policy: Theory and Practice* (New York, 1974); Lawrence Freedman, *U.S. Intelligence and the Soviet Strategic Threat* (Boulder, Colo., 1977); Michael Mandelbaum, *The Nuclear Revolution: The United States and Nuclear Weapons* (Cambridge, 1981); Samuel F. Wells, Jr., "The Origins of Massive Retaliation," *Political Science Quarterly*, 96 (Spring 1981): 31–52; Lawrence Freedman, *The Evolution of Nuclear Strategy* (New York, 1981); Gregg Herken, *Counsels of War* (New York, 1981); David Alan Rosenberg, "A Smoking Radiating Ruin at the End of Two Hours: Documents of American Plans for Nuclear War with the Soviet Union, 1954–1955," *International Security*, 6 (Winter 1981–82): 3–38; John Lewis Gaddis, *Strategies of Containment: A Critical Appraisal of Postwar National Security Policy* (New York, 1982); John Prados, *The Soviet Estimate: U.S. Intelligence Analysis and Russian Military Strength* (New York, 1982); various essays in Graebner, *National Security*; Robert J. Watson, *The Joint Chiefs of Staff and National Policy, 1953–1954* (Washington, D.C., 1986); Brian R. Duchin, "The New Look: President Eisenhower and the Political Economy of National Security" (Ph.D. dissertation, University of Texas, Austin, 1987); Ronald E. Powaski, *March to Armageddon: The United States and the Nuclear Arms Race, 1939 to the Present* (New York, 1987); and

IN TERMS OF NATIONAL-SECURITY PLANNING, the world Eisenhower inherited in 1953 revolved around a position statement drafted three years earlier. National Security Council Paper 68, in many respects the first comprehensive enunciation of American national-security policy, amounted to an American declaration of permanent cold war. The paper rested on two premises: that the decisive struggle in international affairs was between the United States and the Soviet Union and that this contest was a fight to the finish. Moscow, the paper's authors declared, was motivated by "a new fanatic faith, antithetical to our own," and it actively sought "to impose its absolute authority over the rest of the world." Americans faced "a basic conflict between the idea of freedom under a government of laws, and the idea of slavery under the grim oligarchy of the Kremlin." Consequently, for the United States to survive it must prevail, and to prevail it must strengthen its arsenal. The report advocated a massive military build-up and a return to wartime readiness. Such a build-up would be costly; it might, by provoking a Soviet preemptive attack, even be dangerous. "But half-measures will be more costly and more dangerous, for they will be inadequate to prevent and may actually invite war." This was no time for counting expenses. "Budgetary considerations will need to be subordinated to the stark fact that our very independence as a nation may be at stake."⁴

The build-up prescribed by NSC 68 did in fact occur, albeit with a considerable fillip from the North Koreans and the Chinese, who fortuitously confirmed—to Washington's way of thinking, at least—the paper's description of communism as inherently aggressive. From slightly over \$13 billion in 1950, defense spending rose to more than \$50 billion when Eisenhower took office. Philosophically, the triumph of NSC 68 proved commensurately complete, and the embracing anticommunism it proclaimed formed the basis for American strategic planning. The paper's lineal descendant, operative when Eisenhower assumed command in January 1953, averred that recent developments required "no fundamental departures" from the conclusions of NSC 68.⁵

The new president disagreed, and, soon after settling into the White House, he ordered a thorough rethinking of American national-security policy. Eisenhower's fundamental dispute with NSC 68 centered on the high level of military spending Harry S. Truman's blueprint required. Eisenhower had no trouble with the political premise of that paper—that Soviet-led communism posed a

Matthew Evangelista, *Innovation and the Arms Race: How the United States and the Soviet Union Develop New Military Technologies* (Ithaca, N.Y., 1988).

⁴ National Security Council (hereafter, NSC) 68, April 14, 1950, reproduced in Thomas H. Etzold and John Lewis Gaddis, eds., *Containment: Documents on American Policy and Strategy, 1945–1950* (New York, 1978), 385–442. Quotations from 385, 387, 435. The best accounts of the origins of NSC 68 are Paul Y. Hammond, "NSC-68: Prologue to Rearmament," in Schilling, Hammond, and Snyder, *Strategy, Politics, and Defense Budgets*, 267–378; Samuel F. Wells, Jr., "Sounding the Tocsin: NSC 68 and the Soviet Threat," *International Security*, 4 (Fall 1979): 116–58; and Gaddis, *Strategies of Containment*, 89–126. For an insider's view, see Paul Nitze, "The Development of NSC 68," *International Security*, 4 (Spring 1980): 170–76.

⁵ Gaddis, *Strategies*, 359; NSC 135/3, September 25, 1952, *Foreign Relations of the United States, 1952–1954*, vol. 2 (Washington, D.C., 1984), 142–56. Quotation from 144. Henceforth, citations to this series will take the form *FRUS*, 1952–54.

grave danger to the United States—but he rejected the conclusion that this danger required granting *carte blanche* to the Department of Defense. On the contrary, the Republican president, enthusiastically supported by Secretary of the Treasury George Humphrey and Director of the Budget Joseph Dodge, contended that the economic enervation consequent to chronically high defense spending would destroy the country as surely as Soviet bombs. To rein in the Pentagon—to reevaluate, as a White House memorandum stated, “basic national security policies and programs in relation to their costs”—early became a priority.⁶

Former Secretary of State Dean Acheson later wrote that the purpose of the review that led to NSC 68 was “to so bludgeon the mass mind of ‘top government’ that not only could the President make a decision but that the decision could be carried out.” In Eisenhower’s case, the presidential mind did not require a blunt instrument, but the bureaucracy did; accordingly, in May 1953, the president set about fashioning one. He ordered a working committee of the National Security Council, headed by Special Assistant for National Security Affairs Robert Cutler, to appoint consultants to reappraise American national-security policy from first principles. With relative swiftness, Project Solarium, named for the meeting room where the idea of the exercise originated, took shape. Cutler’s committee named three task groups to analyze and present alternative courses of action. Following several weeks of sixteen-hours days at the National War College, the groups reported their findings to Eisenhower and the National Security Council.⁷

Group A, appropriately headed by Soviet expert George F. Kennan, advocated essentially a continuation of the containment policy of the Democrats. Despite the obvious interest of the new administration in reducing defense outlays, Kennan’s group discerned no responsible means for drastically cutting military spending. The Soviet threat had not abated since the adoption of NSC 68; therefore, the United States must keep up its guard. Even with the end of fighting in Korea, the administration ought to budget at least \$40 billion annually for defense. This hard fact precluded the tax cuts many Republicans and others hoped for, but the resulting disappointment was “primarily a political and psychological problem,” not an economic or strategic one. In a nod to Republican orthodoxy, Group A noted the risks attending high taxes and heavy borrowing; yet the members argued that any such risks did “not seem comparable to the Soviet threat.” Fortunately, a light existed at the end of the tunnel. “If we can build up and maintain the strength of the free world during a period of years, Soviet power will deteriorate or relatively decline to a point which no longer constitutes a threat to the security of the United States and to world

⁶ Status of projects reports, February 2 and April 4, 1953, NSC series, Eisenhower Papers (Ann Whitman file), Eisenhower Presidential Library, Abilene, Kansas.

⁷ Dean Acheson, *Present at the Creation: My Years at the State Department* (New York, 1969), 374; Cutler memorandum, May 9, 1953, *FRUS*, 1952–54, 2: 223–24; Bradley to Wilson, July 28, 1953, file 381 U.S. (1–31–50), Joint Chiefs of Staff Records (Record Group 218), National Archives, Washington, D.C.; *Memoirs, 1950–1963* (Boston, 1972); 181–82; Andrew Goodpaster oral history, 2–4, Mudd Library, Princeton University, Princeton, New Jersey.

peace.” For this reason, America could afford to be cautious. American policy should focus on preventing further expansion of Soviet influence rather than on attempting to undo previous gains. American leaders must be prepared for general war if Moscow left them no other choice. But a big war should be a last resort, and the United States ought to make every effort to keep local conflicts local.⁸

Air Force Major General James McCormack, a Pentagon veteran and an expert on atomic weapons, headed Group B, which advocated drawing a “line of no aggression” around the communist bloc and announcing that penetration of this perimeter would result in an atomic attack on the Soviet Union. Option B possessed the principal advantage of simplicity and thrift. The United States only had to prepare for one kind of war. “Since Alternative ‘B’ rules out peripheral wars, its military costs will in the long term be less than the cost of any alternative that accepts such wars—by the amount those wars cost.” The members of McCormack’s group could not quite bring themselves to recommend a reduction in military spending, but they did contend that their proposal allowed the “most economical development and maintenance” of American forces and afforded the greatest readiness in case war came. “The proposed policy will find the U.S. and the free world best prepared to conduct a general war if it is in the Soviet scheme that there must be one.” On the other hand, implementing Option B required clearing a hurdle even its proponents admitted might prove insurmountable. As McCormack summarized:

No satisfactory close-in line was found which would include only the “minimum” areas necessary to U.S. security, without consideration of present obligations, sentiment or past associations . . . No line was found which would exclude any large areas as not absolutely vital to U.S. security, while not discarding the overseas military bases which are so very important for the present, and without banding large industrial resources to the Soviet Union.

Closely related to the difficulty of definition was the question of credibility. While McCormack was describing the necessity of convincing the Soviets that a move against certain territories not intrinsically vital to American security would trigger a major war, Colonel Charles Bonesteel of Group A interrupted: “What if they call our bluff and do move: how do you convince the American people and the U.S. Congress to declare war?” McCormack admitted, “This is a problem.”⁹

Group C, headed by Vice Admiral Richard Conolly, advocated energetic measures to reverse the momentum in the cold war and roll back communist dominion. In marked opposition to Kennan’s group and in lesser contrast to

⁸ Summary of Task Force A report, attached to memorandum by Lay, July 22, 1953, *FRUS*, 1952–54, 2: 399–412. Although Kennan did not relish the idea of defending an assigned position, he took a certain satisfaction in telling Secretary of State John Foster Dulles, who had recently informed him that the new administration would not require his counsel, how to run American policy. “In a curious sense,” Kennan recalled, “at the same time I was dropped from government service, I was asked to write the ticket insofar as policy toward the Soviet Union was concerned”; Kennan oral history, 38, Princeton.

⁹ Summary of Task Force B report, *FRUS*, 1952–54, 2: 412–16; notes of Solarium plenary session, June 26, 1953, *FRUS*, 1952–54, 2: 388–93.

McCormack's, Conolly's team asserted that the Soviet empire did *not* contain the seeds of its own destruction. Quite the reverse: Moscow's strength grew with each passing year. "Time has been working against us. This trend will continue unless it is arrested and reversed by positive action . . . We are convinced that the only way to end the cold war is to face up to the challenge posed by the Communist conspiracy and devote the necessary effort to the task of winning the cold war." While Group B emphasized a military response to the Soviet threat, Group C's members advocated a more comprehensive approach. The United States, they argued, must "prosecute relentlessly a forward and aggressive political strategy in all fields and by all means: military, economic, diplomatic, covert and propaganda." Regarding the last two categories particularly, American leaders should develop a "world-wide covert apparatus" designed "to create the maximum disruption and popular resistance throughout the Soviet bloc." The recommended policy would require a "national program of deception and concealment from public disclosure"; it would infringe civil liberties in the United States, for example, by outlawing the American Communist party; it would strain relations with America's allies; and it would entail "a substantial risk of general war." But the global danger left no room for squeamishness. "In this conflict one is either winning or losing . . . We are still losing."¹⁰

As the reports of the three groups indicated, their approaches, while not fully reconcilable, did include certain areas of overlap; and, following delivery to the National Security Council, the reports went to a committee of representatives of the various interested departments and offices, who were enjoined by the president to combine the best features of each into a statement of policy. The negotiators experienced little difficulty grafting the steady-as-containment-goes philosophy of Option A onto the covert activism of Option C. But the atomic enforcement of Option B, which alone promised the spending restraint Eisenhower, Humphrey, and other fiscal conservatives demanded, occasioned much greater trouble. Unable to reach accord, the drafters delivered a split report to the president.¹¹

THE PROBLEM TURNED ON TWO ISSUES: the respective magnitudes of the military and economic threats confronting the United States and the role of nuclear weapons in American strategy. Regarding the first, the Department of the Treasury and the Bureau of the Budget recognized the need for adequate defenses, but they insisted that the erosion of American economic power that excessive spending and taxation would produce threatened the country more than Soviet military power did, and they demanded explicit acknowledgment of this view in any statement of basic policy. In the Treasury-Budget formulation of

¹⁰ Summary of Task Force C report, *FRUS*, 1952–54, 2: 416–31; Group C summary, July 16, 1953, NSC series, Office of the Special Assistant for National Security Affairs (OSANSA) Records, Eisenhower Library. The latter document, more recently declassified (1987), fills many of the gaps in the former.

¹¹ Status of projects report, September 4, 1953, NSC series, Eisenhower Papers.

NSC 162, the United States would “dangerously weaken its economy” and jeopardize its “free institutions” by running large deficits or significantly raising taxes. The majority of the members of the drafting committee, especially the representatives of the Pentagon and the Department of State, considered this equating of economic risks with military danger illogical and unwise. The majority preferred a simple statement that a “strong, healthy and expanding U.S. economy is essential to the security and stability of the free world.” Beyond this, the majority contended, the administration must not allow fiscal and political pressures to undermine American preparedness. “Without minimizing the strong opposition to high taxation, it is believed that the United States public can be expected to support the requisite measures and expenditures if our security needs are fully understood.”

As Project Solarium’s Group B had recognized, the spending issue related directly to the war-fighting strategy of the United States. Although the authors of NSC 162 managed to reach a unanimous conclusion on military strategy, their recommendation proved just as contentious in the long run as the split opinions regarding military spending. According to the draft paper, American security rested on the ability of the United States “to inflict massive retaliatory damage by offensive strategic striking power” on potential aggressors. Moreover, “the United States should use special [that is, nuclear] weapons whenever they are required by the national security.” In other words, the drafters accepted Group B’s argument that atomic sanctions provided the ultimate and most cost-effective guarantee of American security and that their application ought to be determined primarily by military considerations.¹²

On October 7, the entire National Security Council took up NSC 162. After Robert Cutler’s brief summary of the differences between the Treasury-Budget proposal and the majority opinion, the champions of each position entered the lists. Director of the Budget Dodge asserted that the majority members were ignoring the real and serious danger of runaway spending, despite the fact that “all of us know that it is an objective of Soviet strategy to destroy our capitalist economy by means of economic warfare.” Secretary of the Treasury Humphrey, concurring, noted the necessity of planning for the long term. For brief periods, as during war, the country might allow spending to outstrip income without suffering dire consequences. But what America could survive for a few years would ruin the nation if allowed to persist. “If we mean to face this Soviet threat over a long time, we must spend less than we now are spending and do less than we now are doing.”

Secretary of State John Foster Dulles objected, “with obvious emotion,” in the words of the transcriber of the meeting, to the arguments of Humphrey and Dodge. During subsequent years, Dulles would demonstrate considerable ambivalence, not to say confusion, regarding national-security matters, but in this meeting he declared emphatically that the administration would be irresponsible to throw its defense system “out the window because we had to balance the

¹² NSC 162, September 30, 1953, *FRUS*, 1952–54, 2: 491–514.



Secretary of Defense Charles E. Wilson and Chairman of the Joint Chiefs of Staff Arthur W. Radford, the Pentagon's stalwarts in the fight over nuclear strategy and military spending. May 21, 1956, photo courtesy of United Press.

budget." The secretary of state also complained that, when the topic of spending cuts arose, it was always the defense portion of the bill that came under fire. "Why do we continue spending \$2 billion annually for price supports of agriculture?" Secretary of Defense Charles E. Wilson seconded Dulles, also "speaking with strong conviction." "If we ever go to the American people and tell them that we are putting a balanced budget ahead of national defense, it would be a terrible day." Wilson insisted that American security must override all other considerations. "If we can do this within a balanced budget, fine. If not, we will simply have to postpone balancing the budget."

Throughout this lengthy and divisive meeting, Eisenhower refused to take sides, although he appeared to be leaning in the direction of Humphrey and Dodge. Earlier, the president had made a special point of reminding the Joint Chiefs of Staff of the need for the "maintenance of a sound and strong economy"; now he remarked that the administration was charged with defending "a way of life," not simply territory. "We could lick the whole world if we were willing to adopt the system of Adolph Hitler." Eisenhower agreed with Humphrey that the administration had to develop a policy sustainable over a decade or more. Americans might accept sacrifices in the name of defense for a

few years, but “no eloquence would sell this proposition to the American people for the indefinite future.”¹³

When discussion turned to the use of nuclear weapons, Wilson came straight to the point. The Pentagon’s strategists, he said, “must know whether or not to plan for the use of these weapons. Do we intend to use weapons on which we are spending such great sums, or do we not?” Joint Chiefs of Staff Chairman Arthur Radford reiterated Wilson’s concern. Radford said he was “very worried” about the whole issue of nuclear strategy. He suggested that, unless the United States made clear that it intended to use nuclear weapons whenever military considerations called for their use, it might find itself paralyzed by American and world opinion. The NATO allies in particular, he said, had demonstrated an excessive fear of nuclear weapons; the administration must not grant the Europeans a veto. It was “vital” for the United States to have a free hand in the matter. Dulles echoed Radford’s remarks, saying that “somehow or other we must manage to remove the taboo from the use of these weapons.”

Eisenhower responded that, in one sense, the argument was academic. Regardless of what a policy paper asserted or the allies thought, he as president would personally make any final decision regarding nuclear weapons, and, if American security required their use, he would “certainly decide to use them.” At the same time, he recognized that nuclear weapons constituted a class by themselves. Most of the world, at any rate, regarded them as different from conventional weapons; consequently, Eisenhower said, the United States must exert every effort to secure the “approval and understanding of our allies” before using them. At present, such approval and understanding did not obtain. “Nothing would so upset the whole world as an announcement at this time by the United States of a decision to use these weapons.” Eisenhower declared that the administration would have to bring the allies along carefully. The United States needed overseas bases; in the event of major war, American planes had to be able to hit the Soviets “from any point on the compass.” For this reason, it would be “very undesirable to knock the coalition over the head by precipitate action.”¹⁴

The two issues—the relative importance of military strength and economic health and the role for nuclear weapons—provided grist for discussion at the top levels of the administration during the next three weeks; by the end of October, negotiations had produced a document the president could accept. In the area of spending, the new paper, NSC 162/2, came down on the side of Humphrey and Dodge. As a sop to State and Defense, the budget balancers agreed that the United States must meet “the necessary costs of the policies essential for its security,” and they conceded that, for a “limited period,” certain “overriding national objectives” might justify a departure from “sound fiscal policies.” But such exceptions must be transitory. In the long run, the United States had no

¹³ Memorandum of discussion at 165th NSC meeting, October 7, 1953, *FRUS*, 1952–54, 2: 514–34; Cutler to Bradley, July 24, 1953, 381 U.S. (1–31–50), JCS Records.

¹⁴ Memorandum of discussion at 165th NSC meeting, October 7, 1953, *FRUS*, 1952–54, 2: 514–34.

choice but to “maintain a sound economy based on free enterprise as a basis both for high defense productivity and for the maintenance of its living standards and free institutions.” In what was remarkably specific economic language for a paper devoted to broad national-security policy, NSC 162/2 declared:

Excessive government spending leads to inflationary deficits or to repressive taxation, or to both. Persistent inflation is a barrier to long-term growth because it undermines confidence in the currency, reduces savings, and makes restrictive economic controls necessary. Repressive taxation weakens the incentives for efficiency, effort, and investment on which economic growth depends.

Although Dulles, Wilson, and Radford lost to Humphrey and Dodge on the spending issue, the first three won on the matter of nuclear weapons. As did the draft version, NSC 162/2 premised American security on the development and maintenance of “a strong military posture, with emphasis on the capacity of inflicting massive retaliatory damage by offensive striking power.” The new paper went further, strengthening the language governing the use of nuclear weapons: “In the event of hostilities, the United States will consider nuclear weapons to be as available for use as other munitions.” Naturally, the allies should be consulted regarding American nuclear policy, and, where authorization by an allied government was required for the use of nuclear weapons from a base on foreign soil, the United States “should promptly obtain the advance consent of such ally for such use.” But the United States must not allow itself to be hamstrung by adverse world opinion. “U.S. leadership in this regard . . . does not imply the necessity to meet all desires of our allies.”¹⁵

BY SETTING HIS SEAL TO NSC 162/2, Eisenhower completed a process that, in appearance at least, effected a revolution in American security planning. On paper, the administration had solved two of the outstanding problems of national defense: how to safeguard the country without bankrupting it and what to do with nuclear weapons. Unfortunately, the mastery of the bureaucratic process Eisenhower demonstrated during his first year in office soon faded. Unable to make the decisions of NSC 162/2 stick, the president spent much of the next seven years re-fighting the revolution, with diminishing success.

Part of the problem lay with the new policy itself. A deterrent, nuclear or otherwise, must be believable to be effective. Eisenhower’s “New Look” immediately ran into difficulties on this score. In January 1954, John Foster Dulles provided the clearest public statement of the policy’s rationale. Speaking to the Council on Foreign Relations, Dulles likened the United States to a policeman. “We keep locks on our doors but we do not have an armed guard in every home. We rely principally on a community security system so well equipped to punish any who break in and steal that, in fact, would-be aggressors are generally deterred. That is the modern way of getting maximum protection at a bearable cost.” America could not afford to place troops in all the world’s trouble spots.

¹⁵ NSC 162/2, October 30, 1953, *FRUS*, 1952–54, 2: 577–97.

"If our policy was to remain the traditional one of meeting aggression by direct and local opposition, then we needed to be ready to fight in the Arctic and in the Tropics; in Asia, the Near East, and in Europe; by sea, by land, and by air; with old weapons and with new weapons." To avoid this crushing burden, which would produce "grave budgetary, economic, and social consequences" for the United States, the Eisenhower administration had made a "basic decision": "to depend primarily upon a great capacity to retaliate, instantly, by means and at places of our choosing." The result was a policy allowing the United States to develop "more basic security at less cost."¹⁶

Albert Einstein once recommended making complicated matters as simple as possible but no simpler. Dulles's speech on what headline writers quickly dubbed "massive retaliation" demonstrated the dangers of ignoring this advice. Dulles meant to describe a policy by which the United States would *increase* its options in the event of communist aggression, by giving the administration the capacity to retaliate "by means and at places of our choosing." But, in striving for effect, Dulles came across as issuing an all-or-nothing challenge of the sort the National Security Council had explicitly rejected during the Solarium exercise, and he provoked a storm of controversy that lasted much of the decade.¹⁷

Difficulties of communication aside, the massive-retaliation policy suffered from one fatal weakness: it required the United States to risk national suicide for interests not inherently vital to American security. Even as the Solarium review took place, American intelligence analysts described the growing vulnerability of the United States to Soviet atomic attack. In January 1953, a special study for the

¹⁶ Department of State *Bulletin* (January 25, 1954): 107–10. For an early expression of Dulles's thinking on massive retaliation, see "A Policy of Boldness," *Life*, 32 (May 19, 1952): 146–60. A more thoughtful, less politicized tone characterized a memorandum on the same subject, written just after Dulles took office. Dulles memo, undated (early 1953), White House memoranda series, John Foster Dulles Papers, Eisenhower Library. In his emphasis on "the long pull," Dulles indicated his belief that time was diminishing the dynamism of the Soviets' foreign policy. As Eisenhower aide Andrew Goodpaster recalled, the secretary of state thought that "in due course they would turn their ponies to the East and head back as Attila had done"; Goodpaster oral history, 2–4, Princeton.

¹⁷ Robert Bowie, head of the NSC Policy Planning Board, thought the "massive retaliation" address typical of Dulles's ability to convey the wrong message and to appear more inflexible than he actually was. "I don't think that the speech was an adequate representation of his own thinking," Bowie commented later. "But it was an excellent example of what I am trying to stress. And that is that in speaking he was so anxious to get things simple and clear and forceful and to have them get attention, that he gave a picture of a mind which has all these qualities—of simplification in black and white"; Bowie oral history, 42, Princeton. Other administration officials set about trying to correct the impression Dulles created. Arthur Radford, for example, adopted the line that the New Look marked no great change in American strategy. As the Joint Chiefs of Staff chairman explained to a group of Canadian defense planners, the new approach amounted simply to preparation "for the long pull in lieu of a year-of-crises"; Notes for Radford meeting with Canadian officials, March 4, 1954, 381 Continental Defense, Radford files, JCS Records.

Among the more important contributions to the debate Dulles aroused were Bernard Brodie, "Unlimited Weapons and Limited War," *Reporter*, 11 (November 18, 1954): 16–21; and *Strategy in the Missile Age* (Princeton, N.J., 1959); William W. Kaufmann, "The Requirements of Deterrence," in Kaufmann, ed., *Military Policy and National Security* (Princeton, N.J., 1956), 12–38; P. M. S. Blackett, *Atomic Weapons and East-West Relations* (Cambridge, 1956); Matthew B. Ridgway, *Soldier* (New York, 1956); Henry A. Kissinger, *Nuclear Weapons and Foreign Policy* (New York, 1957); Robert Osgood, *Limited War: The Challenge to American Strategy* (Chicago, 1957); Albert Wohlstetter, "The Delicate Balance of Terror," *Foreign Affairs*, 37 (January 1959): 211–34; Oskar Morganstern, *The Question of National Defense* (New York, 1959); and Maxwell D. Taylor, *The Uncertain Trumpet* (New York, 1959). For a summary of the debate, see Paul Peeters, *Massive Retaliation: The Policy and Its Critics* (Chicago, 1959).

National Security Council warned of the extreme porosity of American defenses against a Soviet air offensive. NSC 141 set the effectiveness of the American shield at no more than 15 percent, which translated into a 75 percent ratio for Soviet bombs-on-target. Shortly after the Soviets exploded a hydrogen device in August 1953, administration experts predicted that, within a few years, a general war between the United States and the Soviet Union “would bring about such extensive destruction as to threaten the survival of Western civilization.” The staff of the National Security Council, assessing the possibility of defending America, concluded not only that present American defenses were inadequate “to prevent, neutralize or seriously deter” Moscow from attacking the United States but that *any* defensive system approaching the effectiveness Americans until recently had taken for granted was “probably unattainable” and was “completely impractical, economically and technically, in the face of expected advances in Soviet offensive capabilities.”¹⁸

As time passed, the news only grew grimmer. In June 1954, National Security Council analysts revised sharply upward their estimates of Soviet nuclear capabilities. At the beginning of 1955, a special committee headed by James Killian, president of the Massachusetts Institute of Technology, produced a multivolume report on the dangers of a Soviet nuclear attack. The language of the Killian paper was chilling. Citing the possibility of “death and destruction on a scale almost beyond knowing, and certainly beyond any sensibility to shock and horror that men have so far experienced,” the report declared that, “for the first time in history, a striking force could have such power that the first battle could be the final battle, the first punch a knockout.” Shortly after Eisenhower received this sobering assessment, the National Security Council staff asserted that, while an increase in tension and some mobilization would probably precede a nuclear attack, “the possibility of total surprise cannot be excluded.” In June 1955, the Central Intelligence Agency recalculated its figures on Soviet uranium production and stated that the Kremlin’s weapons makers would soon possess more than three times the fissionable material predicted earlier.¹⁹

An attack by Soviet bombers posed the principal threat to American security during most of the 1950s, and, despite efforts to strengthen American and Canadian defenses, a standing committee appointed by Eisenhower to monitor

¹⁸ NSC 141, January 19, 1953, *FRUS*, 1952–54, 2: 209–22; NSC 159/4, September 25, 1953, *FRUS*, 1952–54, 2: 475–89; “Interim Defense Mobilization Planning Assumptions,” undated, attached to Lay to NSC, November 20, 1953, *FRUS*, 1952–54, 2: 605–07. Also Jackson memorandum, May 20, 1953, C. D. Jackson Papers, Eisenhower Library; Savage memorandum, February 10, 1953, *FRUS*, 1952–54, 2: 231–34; Carns memo, October 20, 1953, 381 Continental Defense, Radford files, JCS Records; Radford memo, October 21, 1953, 381 Continental Defense, Radford files, JCS Records; Joint Strategic Plans Committee (JSPC) 912/42, March 20, 1953, 381 U.S. (5–23–46), JCS Records; Howe to Smith, March 1, 1954, *ibid.*, 634; Joint Intelligence Committee (JIC) 382/30, January 7, 1953, 381 U.S. (5–23–46), JCS Records. Commenting on this and other similarly worrisome estimates, Air Force Chief of Staff Nathan Twining later recalled that “we were really scared of the Russians . . . Our people were really frightened of Russia, of an attack”; Twining oral history, 8–9, Princeton.

¹⁹ NSC 5422, June 14, 1954, *FRUS*, 1952–54, 2: 647–67; Technological Capabilities Panel (Killian) report, February 14, 1955, Office of the Staff Secretary Records, Eisenhower Library; NSC 5515, March 21, 1955, NSC series, OSANSA Records; Allen Dulles to Cutler, June 8, 1955, NSC series, Eisenhower Papers.

Soviet capabilities reported at the end of 1957 that the continental defense system possessed “a number of ‘Achilles’ heels’ which can be exploited by an intelligent enemy.” Among these were electronic countermeasures designed to fool American radar, saturation attacks to overwhelm it, and low-altitude flight paths to elude it until the last moment.²⁰

As the end of the decade approached, American analysts became increasingly concerned with the danger posed by another Soviet delivery system, intercontinental ballistic missiles, or ICBMs. Until 1953, doubt existed, among American weapons designers at least, that hydrogen warheads could be sufficiently miniaturized to fit on long-range missiles. At the beginning of the following year, however, the administration’s technical experts informed the National Security Council that American scientists had solved the basic problems. Recent experience indicated that the Soviets must not be far behind; current intelligence suggested they might even be ahead. The prospect of Soviet rockets raining bombs from the heavens added enormously to American security worries. As John von Neumann of the Atomic Energy Commission explained to the National Security Council at a meeting in September 1955, the great speed of the ICBMs allowed Washington only fifteen minutes warning between detection and detonation. Further, under even the most optimistic assumptions, ICBMs would be “horribly difficult” to intercept. Finally, ICBMs were relatively cheap—on the order of \$1 million each, von Neumann said—and the Soviets could produce them in large numbers.²¹

GROWING SOVIET STRENGTH eroded public’s confidence in the massive-retaliation policy; meanwhile, Eisenhower’s weak leadership was compromising the policy in the inner circles of government. The president’s acceptance of NSC 162/2 should have settled the twin questions of military versus economic strength and nuclear versus conventional forces, but it did not. Eisenhower failed to enforce his decision, allowed the debate to continue, and so prepared the way for the eventual disintegration of the policy.

Reconsideration of the New Look began almost before NSC 162/2 returned from the printers. Initially, the reconsideration involved not the decisions set forth by that document but issues consequent to those decisions and to projected developments in Soviet military power. Moscow’s impending achievement of a

²⁰ NSC 5605, May 24, 1956, NSC series, OSANSA Records; NSC 5724/1, December 16, 1957, NSC series, OSANSA Records.

²¹ Memorandum of discussion at 258th NSC meeting, September 8, 1955 (memo dated September 15, 1955), NSC series, Eisenhower Papers. Simon Ramo, a colleague of von Neumann and a consultant to the Department of Defense during this period, described the intelligence reports on Soviet progress in missile development in *The Business of Science: Winning and Losing in the High-Tech Age* (New York, 1988), 78–82. See also Prados, *Soviet Estimate*, 57–63; and Freedman, *U.S. Intelligence*, 68–72. Ramo noted one of the most frightening aspects of Soviet ICBM development at the time: the inability of American radar to detect warheads coming from above the atmosphere.

While the administration spent most of its energy worrying about airplane and missile attacks, the Joint Chiefs pointed out that the Soviets might achieve comparable surprise using submarines or by the clandestine introduction of an atomic device into the United States, perhaps concealed in the hold of a cargo ship. JCS 1899/53, August 8, 1953, 381 U.S. (5–23–46), JCS Records.

capacity to destroy the United States forced American planners to revise the method by which they calculated the Soviet threat. Under the regime of NSC 68, American strategists had focused primarily on Soviet capabilities rather than on the intentions of the Soviet leaders. While Moscow's intentions were unknowable, prudence dictated preparing for the worst. Besides, emphasizing what the Kremlin could do, as distinct from what it would do, enhanced the bludgeoning effect that was NSC 68's *raison d'être*.

After Eisenhower entered office, policy makers did not neglect Soviet capabilities—witness the daunting forecasts just described—but they placed greater emphasis on the Kremlin's intentions. To a certain degree, this shift reflected political concerns. In justifying his plan to trim the defense budget, Eisenhower had to beware of what he called the “very vociferous minority” in Congress who were “violently opposed to even the slightest cuts” in national-security programs. Disgruntled military officers, especially in the army, which bore the brunt of the New Look cuts, could be expected to abet such opposition. (They did.) At a moment when Soviet capabilities were indisputably increasing, justifying reductions in spending required reducing the threat; the obvious way to accomplish this was to contend that the Soviets *would* do less than they *could*.²²

Beyond the bureaucratic imperative, the nation-destroying magnitude of the Soviet threat almost forced American officials to concentrate on Moscow's intentions. From an existential standpoint, living with the knowledge that the Soviets would soon be able to decimate the United States required finding some reason to think this fate would not descend on the country.

To an entirely unprecedented degree, the continuation of life in the United States was coming to depend on the cooperation of the Kremlin. In the absence of physical barriers against a Soviet atomic attack, the United States had to rely on a psychological defense, namely, the threat of retaliation. The Soviets could destroy the United States, but they must be made to choose otherwise, out of fear they in turn would be obliterated. Since deterrence cut both ways, the result was

²² Memorandum of discussion at 140th NSC meeting, April 22, 1953, *FRUS*, 1952–54, 2: 291–301. Gaddis, *Strategies*, 139 and following, also commented on the shift from capabilities to intentions; but he saw it as following less from technological developments than from Dulles's concentration on the ideological roots of Soviet behavior.

Regarding the army's opposition to Eisenhower's budget trimming, a memorandum of March 1953 gave an indication of what the president could expect. Entitled “Statement of Effect on Army Programs of an Expenditure Limitation of 14.9 Billion Dollars in FY 1954 and 13.2 Billion Dollars in FY 1955,” the paper described all manner of dire consequences that would follow the imposition of such spending limits. On microfilm in *Declassified Documents Reference System* (Washington, D.C., 1975–), 87/667. The army argued as well that nuclear weapons did not make conventional forces obsolete. See Lyman Lemnitzer to Mathewson, February 9, 1955, 381, Radford files, JCS Records; see also “A Dynamic Role for the Army in Atomic Warfare,” in Watson to Radford, March 29, 1954, 381, Radford files, JCS Records.

Army Chief of Staff Matthew Ridgway led the opposition to the New Look, complaining to sympathetic legislators that the Eisenhower administration was starving the army. His actions naturally incensed his superiors. Joint Chiefs of Staff Chairman Radford later recalled, “Had his position been that of an isolated individual it would not have mattered, but he was the army chief of staff. He disturbed the President and the Secretary of Defense. He certainly disturbed me”; Stephen Jurika, ed., *From Pearl Harbor to Vietnam: The Memoirs of Admiral Arthur W. Radford* (Stanford, Calif., 1980), 329.

an implicit non-irrationality pact between Washington and Moscow, based on estimates by each of the other's intentions.²³

While America's safety might rest on the Soviets' good sense, no one in Washington contended it should rely on Moscow's good will. All agreed that the United States could only guarantee deterrence—and even then the guarantee would be no more than partial—by safeguarding America's deterrent. But dispute soon arose regarding what this required. Not surprisingly, in light of the vigorous debate over the earlier question of how much offense constituted a good defense, and what the United States should pay for it, violent differences marked discussions of how much defense a good offense required. Perhaps equally predictably, the losers in the fight over NSC 162/2 took this new round of bureaucratic combat as an opportunity to try to reopen the issues apparently settled by that paper.

As before, the struggle turned on the wording of a draft position statement. NSC 5422 bore the innocuous title "Tentative Guidelines under NSC 162/2 for Fiscal Year 1956" and purported simply to provide recommendations for implementing the policy set forth in the earlier paper. In reality, NSC 5422 amounted to a fundamental reassessment of the policy itself. Unable to resolve the issue of adequate defense against nuclear attack, the paper's drafters delivered another split opinion. The hard-liners called for "whatever measures are necessary" to protect the American retaliatory force against "any foreseeable Soviet attack" and characterized the related expenditures as nothing less than "a prerequisite to U.S. survival." The budget balancers insisted on nothing more than "all practicable measures" for defending the arsenal.

The importance of protecting the nuclear arsenal related closely to the role of nuclear weapons in American planning. In the negotiations over NSC 5422, the groups dissatisfied with the New Look succeeded in placing back on the bargaining table the question of the role of nuclear weapons. The predictions of American vulnerability to Soviet attack weakened the case for massive retaliation and played into the hands of army officers upset at their eclipse by colleagues commanding strategic weapons. The army counterattacked by insisting that the United States must strengthen its ability to fight limited wars, using either conventional arms or tactical nuclear weapons or both. The Pentagon hierarchy, hoping to quell a mutiny in the ranks, went along. Without backing away from its commitment to strategic arms, the Department of Defense advocated strengthening America's sub-strategic arsenal. Thus the Defense draft of NSC 5422 asserted:

In the face of possible nuclear balance in 1956–59, there is serious question whether the U.S., while maintaining maximum strategic nuclear capabilities, can continue to place major reliance thereon as a means of waging general war. Consequently, the U.S. should

²³ American war planners, while assuming a basic rationality on the part of Soviet leaders, could not rule out an accidental war. "It is improbable that the U.S.S.R. will deliberately initiate general war against the U.S. in the next two years," the Pentagon's Joint Strategic Plans Committee asserted in 1954, but the committee added the important qualification that international tension "could degenerate into general war from miscalculations by either or both countries"; JSPC 842/129, January 6, 1954, 381 U.S. (5–23–46) JCS Records.

undertake to increase the forces and mobilization potential which the U.S. and its allies would need to wage war effectively without strategic use of nuclear weapons.

The budget balancers disagreed strongly. Considering the argument from stalemate essentially a device for subverting the economies they had won via NSC 162/2, the Humphrey-Dodge contingent refused to blink in the face of a potential holocaust. Their draft of NSC 5422 declared:

Despite the advent of nuclear balance, the U.S. must accept the risks involved in relying upon strategic nuclear capabilities as a means of waging general war, and must employ its scientific knowhow and industrial superiority to maintain qualitative advantage over the Soviets. The U.S. must continue to make clear its determination to meet Soviet attack with all available weapons. Only in this way can there be a maximum deterrent to general war, which if it comes will in all probability involve the unrestricted use of nuclear weapons.

The draft paper also split regarding efforts to cap the arms race through negotiation, which at least in theory offered a means of slowing the development of American vulnerability. One version, favored by the Department of State, argued that the administration should explore the possibility of "a practicable arrangement for the limitation of armaments with the USSR," since such an arrangement would be "a more certain and economical method of meeting the threat posed by the growing Soviet nuclear capability" than any other likely course. The Pentagon's version rejected disarmament, on grounds that the Soviets would cheat. "There is serious question," it asserted, "whether any safe and enforceable system for the limitation of armaments can be achieved, so long as the Soviet regime and objectives remain substantially as they are today."²⁴

Finally, the NSC 5422 draft reargued the issue of how much weight to give to the worries of the allies in deciding for or against the use of nuclear weapons. No one doubted that the growth of Soviet power would test the resolve and cohesiveness of NATO. "It may be," Commandant of the Marine Corps Lemuel Shepherd had written to the Joint Chiefs of Staff, "that there is a point at which U.S. forces and bases on foreign soil will come to be regarded less as a deterrent to war than a threat to peace." The Joint Chiefs of Staff intelligence board concurred, predicting that "by mid-1955 both the Soviet Bloc and the Western Powers will possess stockpiles of weapons of mass destruction, the threatened use of which might induce some countries presently aligned with the Soviets or the West to seek a different status." Given the current state of technology, the loss of allies would seriously undermine America's deterrent. With ICBMs at least half a decade in the future, the United States required forward bases for quick blows at the Soviet heartland. Indeed, a paper prepared for the National Security Council Planning Board described the acquisition and retention of "useable missile bases and air fields within early striking range" of the Soviet Union as "the most important strategic problem" confronting the United States.²⁵

²⁴ NSC 5422, June 14, 1954, *FRUS*, 1952-54, 2: 647-67.

²⁵ Shepherd to JCS, December 5, 1953, 381 U.S. (1-31-50), JCS Records; JIC 382/30, January 7, 1953, 381 U.S. (5-23-46), JCS Records; Elliot to NSC Planning Board, October 20, 1955, 381 U.S. (1-31-50), JCS Records. For background on American interest in overseas bases, see Melvyn P. Leffler, "The American Conception of National Security and the Beginnings of the Cold War, 1945-48," *AHR*, 89 (April 1984): 346-81.

Yet, crucial as the allies were, their susceptibility to what the National Security Council Planning Board called “atomic blackmail” jeopardized the ability of the United States to act forcefully when American interests required. The drafters of NSC 5422, unable to agree on the optimal balance between cooperation and unilateralism, offered still another split opinion. The more alliance-oriented version, reflecting Department of State views, advocated the development of “maximum self-sufficiency” consistent with sound military principles and left the matter at that. The Pentagon’s representatives, unwilling to take dictation from the Europeans, accepted self-sufficiency for the future but declared that, in the meantime, the United States must make plain its willingness to act “unilaterally if necessary” against Soviet-sponsored aggression, even to the point of general war.²⁶

The circulation of the draft of NSC 5422 touched off a new round of skirmishes within the administration, with the contest climaxing in a National Security Council meeting in June 1954. After Robert Cutler summarized the divergences between the drafts, Eisenhower expressed annoyance that the questions occasioning the splits were still being debated. Referring specifically to the issue of emphasizing nuclear or conventional weapons, the president said he thought the matter had been settled already, in favor of assuming the availability of nuclear weapons. It was “simply impossible,” he declared, “to try to play safe in all the possible kinds of warfare.” Should the administration accept the argument of the limited-war partisans, “we might just as well stop any further talk about preserving a sound U.S. economy and proceed to transform ourselves forthwith into a garrison state.” Arthur Radford agreed. The Joint Chiefs of Staff chairman, while supporting the army’s desire for stronger conventional forces, did not wish to forfeit the use of nuclear weapons. He said that if the administration decided at this late date to retreat from its nuclear commitment, American strategy would have to be “completely changed.” For a different, namely budgetary, reason, Secretary of the Treasury Humphrey also urged that the previous decision in favor of nuclear weapons be reaffirmed. Humphrey reminded Eisenhower that the National Security Council had “long since” decided the question. Now, he said, it was “high time that this decision was enforced.”

When Cutler responded that the Planning Board had raised this “hoary issue” because the approaching nuclear stalemate might, in the view of some of the board’s members, enable the Soviet Union to avoid general war and yet “nibble the free world to death piece by piece,” Eisenhower objected strenuously. Such a view, the president declared, was “completely erroneous.” Precisely the opposite was likelier to occur. “The more atomic weapons each side obtains,” he said, “the more anxious it will be to use these weapons.” Some people had argued from analogy with chemical weapons during World War II, when fear of retaliation had prevented the losing Germans from employing poison gas against the advancing allies. Eisenhower asserted that the analogy did not apply

²⁶ NSC 5422, June 14, 1954, *FRUS*, 1952–54, 2: 647–67.

and that the United States would err to think the Soviets would not use nuclear weapons in a future war. In any event, the president warned against placing too much significance on abstract decisions made in the context of a National Security Council planning session. Crises, when they occurred, would be dealt with on their concrete and individual merits, not according to the guidelines of some general position paper.²⁷

BUT THIS WAS NO ANSWER. Choices made in advance of crises would determine the options available at the moment of decision. Especially in the realm of military strategy, where extended lead times characterize the development of force postures, policies determined by default count fully as much as those formulated by design. Eisenhower could say he would wait until the last moment to choose for or against nuclear weapons, but, at the moment circumstances demanded a choice, he might find that intervening developments had narrowed his alternatives.

John Foster Dulles recognized that the administration's inaction had already reduced its freedom of maneuver. In the National Security Council meeting just described, the secretary of state observed that resistance among the allies to a firm anticommunist line was growing by the month. The recent failure of the British to back American intervention in Indochina was a case in point. Dulles worried that the Europeans would prove even more reluctant to support the United States if Washington adopted a strategic policy like that advocated by the Joint Chiefs of Staff: one aiming to exploit America's nuclear superiority over the Soviet Union. "Very few of our allies will follow us," Dulles said. "They will follow those who say 'let's not be tough and let's not press our issues with Russia' . . . The tide is clearly running against us in the channel of this tough policy. If we are to continue to pursue it we shall lose many of our allies . . . We must recognize the fact that we can no longer run the free world, and accordingly review our basic security policy."²⁸

After this meeting, the National Security Council Planning Board did its best to reconcile the divergent views, and, during six weeks more of haggling, it managed to cobble together a paper Eisenhower could accept. On two of the issues in dispute, the presidential nod went to the budget balancers. Despite the fact that the Soviets would soon be able to counter any American nuclear threat with one of their own, Eisenhower authorized American planners to operate on the assumption that, if general war broke out, the United States would wage it with "all available weapons." As to defending the deterrent, the administration confined itself to "practicable measures." On two other issues—reliance on allies and disarmament—the paper equivocated. The administration would do its best to obtain maximum support from the allies, and it would pursue its objectives in such a manner as to "maintain the cohesion of the alliances." But the United

²⁷ Memorandum of discussion at 204th NSC meeting, June 24, 1954, *FRUS, 1952–54*, 2: 686–98.

²⁸ *Ibid.*

States would “act independently of its major allies when the advantage of achieving U.S. objectives by such action clearly outweighs the danger of lasting damage to its alliances.” Regarding arms control, the administration would “continue to reexamine its position on disarmament,” toward the goal of discovering whether “a system of safeguards can be devised entailing less risk for the U.S. security than no limitation of armaments.”²⁹

Yet even this reiteration of policy, confirming what the president had decided earlier, failed to still the debate within the administration. As before, the role of nuclear weapons provided the source of contention. By affirming that the United States would wage general war with “all available weapons,” the president simply ratified a tautology: any war in which Washington held back strategic weapons would not be “general.” Eisenhower continued to beg the question of what interests—particularly, what territories—the administration considered worth the risk of all-out war. Eisenhower’s approval of NSC 5422 simply papered over the problem Group B of Project Solarium had been unable to settle: which countries and regions the United States ought to defend to the deaths of millions of Americans.

Dulles, for one, was beginning to doubt whether any place outside the United States justified such a price. “The increased destructiveness of nuclear weapons and the approach of effective atomic parity,” the secretary wrote in November 1954, “are creating a situation in which general war would threaten the destruction of Western civilization.” Dulles argued further that “national objectives could not be attained through a general war, even if a military victory were won.” Asserting that “total war would be an incalculable disaster,” the secretary maintained that the United States must forgo “actions which would generally be regarded as provocative” and that it must “be prepared, if hostilities occur, to meet them, where feasible, in a manner and on a scale which will not inevitably broaden them into total nuclear war.” The administration, Dulles concluded, must “avoid risks which do not promise commensurate strategic or political gains.”³⁰

Coming from the principal exponent of massive retaliation and the man who would boast of his skills at brinkmanship, this was astonishing counsel. Dulles’s second thoughts on the efficacy of nuclear diplomacy would be followed by third and fourth thoughts, which would carry the secretary back toward his original position and then away once more. In the meantime, the Department of Defense, sensing a softening on the part of a hitherto staunch supporter, registered a sharp protest.

The Pentagon, even as it argued for increases in conventional and tactical nuclear forces, rejected any curbs on the availability of strategic weapons. The Joint Chiefs of Staff, the service secretaries, and Secretary of Defense Wilson dismissed the notion that the great destructive power of strategic arms rendered them unusable. Lest anyone get the wrong idea, the military leaders said they had ruled out “preventive war or acts intended to provoke war”; but, still,

²⁹ NSC 5422/2, August 7, 1954, *FRUS*, 1952–54, 2: 715–31.

³⁰ Dulles memo, November 15, 1954, *FRUS*, 1952–54, 2: 772–76.

Pentagon officials discerned a “wide latitude” for American action in the face of the Soviet threat. They denied Dulles’s assertion that general war would be an unmitigated disaster. On the contrary, they declared, the United States not only could survive a nuclear war, it could win such a war “beyond any reasonable doubt.” To guarantee this possibility, however, the administration must shift from the negative approach of merely reacting to Soviet moves to “a basic U.S. security policy of unmistakably positive quality.” Without specifying the precise nature of the “timely” and “dynamic” actions they desired—these would be decided in individual cases according to “what best serves the interests of the United States”—the Pentagon officials urged that the administration prepare to take “certain risks inherent in the adoption of dynamic and positive security measures.”³¹

Arthur Radford amplified the Department of Defense’s view in a November 1954 National Security Council meeting. Referring to Indochina, the Joint Chiefs of Staff chairman blamed the communist victory there on a “lack of courage” on the part of the British—and implicitly on the president’s failure to override Britain’s veto. Radford came close to advocating preemptive war in claiming that, if a general conflict occurred, the United States would win, but only “prior to Soviet achievement of atomic plenty,” which he expected by the end of the decade. A policy of caution, Radford suggested, did not lessen risks; it merely postponed them. “The Joint Chiefs feel that if we continue to pursue a policy of simply reacting to Communist initiatives, instead of a policy of forestalling Communist action, we cannot hope for anything but a showdown with Soviet Communism by 1959 or 1960.”³²

Radford’s remarks came against a background of deepening crisis in the Taiwan Strait. In September 1954, artillery units of the People’s Republic of China began shelling positions held by the Republic of China on the island of Jinmen (Quemoy). During the next several weeks, naval and air forces on both sides entered the fighting. Although the Eisenhower administration succeeded in persuading Nationalist President Chiang Kai-shek to retreat from the most indefensible of Taiwan’s offshore territories, Chiang managed to extract a mutual-defense treaty from Washington. Meanwhile, Eisenhower spread responsibility for whatever might follow by securing a congressional resolution authorizing the president to commit American military power to the defense of the Taiwan area. Did this include Jinmen and the other offshore islands? The administration was not saying; it had not decided.³³

³¹ Wilson to Lay, November 22, 1954, *FRUS*, 1952–54, 2: 785–87.

³² Memorandum of discussion at 225th NSC meeting, November 24, 1955, *FRUS*, 1952–54, 2: 787–802. Curtis LeMay, the outspoken chief of the Strategic Air Command, later put SAC’s capacity for knocking out the Soviet Union more directly, saying his bombers could have destroyed “all of Russia (I mean by that all of Russia’s capability to wage war) without losing a man to their defenses”; Thomas M. Coffey, *Iron Eagle: The Turbulent Life of General Curtis LeMay* (New York, 1986), 331. On the issue of Pentagon thinking on preemptive strikes, see Rosenberg, “Smoking, Radiating Ruin,” 13–16.

³³ On the Taiwan Strait affair, see O. Edmund Clubb, “Formosa and the Offshore Islands in American Policy,” *Political Science Quarterly*, 74 (December 1959): 517–31; Morton H. Halperin and Tang Tsou, “United States Policy toward the Offshore Islands,” *Public Policy*, 15 (1966): 119–38; Leon V. Sigal, “The ‘Rational Policy’ Model and the Formosa Straits Crisis,” *International Studies*

By March 1955, the administration found itself in a corner. To concede the islands to the communists, in case Beijing pressed the attack, would damage American prestige, not to mention setting off an uproar among Chiang's American supporters. To defend the islands successfully would almost certainly require the use of nuclear weapons and might force the Soviets to enter the fray. After a visit to Taipei, Dulles reported to Eisenhower and the National Security Council that the administration must ready itself for a "quite serious showdown." The secretary said that unless the administration abandoned Chiang, there existed "at least an even chance" that American forces would have to fight. Indeed, Dulles went so far as to characterize war as "a question of time rather than a question of fact," and he concurred with Pentagon predictions that such a conflict would quickly lead to the use of nuclear weapons. Preparing for the worst, Eisenhower directed the military to set war plans in motion. The Joint Chiefs responded by giving the commander of the U.S. Seventh Fleet authority to conduct reprisals against mainland installations in the event of attacks on American ships and by ordering the Strategic Air Command to begin, on an "urgent basis," target selection for an "enlarged atomic offensive" against the People's Republic of China.³⁴

Throughout all this, Radford argued that troubles in the Taiwan Strait transcended America's relationship with East Asia. The Joint Chiefs of Staff chairman contended that they represented a fundamental test of American national-security policy. Would the New Look become a strategic reality, he asked, or would it remain simply an excuse for cutting defense costs? In March 1955, he reminded Eisenhower of the president's repeated assertions regarding the availability of nuclear weapons. America's "whole military structure," Radford said, had been built around the assumption that in a fight with the communists, the United States would use nuclear weapons. He went on to predict that troubles with the Chinese would not cease until Beijing got "a bloody nose." To this end, he called for preemptive strikes against airfields in the People's Republic of China and an explicit warning to Beijing and Moscow that

Quarterly, 14 (June 1970): 121–56; George and Smoke, *Deterrence*, chap. 9; J. H. Kalicki, *The Pattern of Sino-American Crises: Political-Military Interactions in the 1950s* (London, 1975); Hungdah Chiu, "The Question of Taiwan in Sino-American Relations," in Hungdah Chiu, ed., *China and the Taiwan Issue* (New York, 1979), chap. 6; Bennett C. Rushkoff, "Eisenhower, Dulles, and the Quemoy-Matsu Crisis, 1954–1955," *Political Science Quarterly*, 96 (Fall 1981): 465–80; Thomas E. Stolper, *China, Taiwan, and the Offshore Islands* (Armonk, N.Y., 1985); Leonard H. D. Gordon, "United States Opposition to Use of Force in the Taiwan Strait, 1954–1962," *Journal of American History*, 72 (December 1985): 637–60; Gordon H. Chang, "To the Nuclear Brink: Eisenhower, Dulles, and the Quemoy-Matsu Crisis," *International Security*, 12 (Spring 1988): 96–122; and H. W. Brands, Jr., "Testing Massive Retaliation: Credibility and Crisis Management in the Taiwan Strait," *ibid.*, 124–51.

³⁴ Minutes of cabinet meeting, March 11, 1955, Eisenhower Papers; memorandum of discussion at 240th NSC meeting, March 10, 1955, *FRUS, 1955–57*, 2: 345–50; Robert Totten for Joint Intelligence Group to Radford, March 16, 1955, 091 China, Radford files, JCS Records; Radford for JCS to Twining, *et al.*, February 5, 1955, Combined Chiefs of Staff (hereafter, CCS) 381, JCS Records; CINCPAC to USARPAC *et al.*, January 30, 1955, CCS 381, JCS Records; JCS to COMSAC, February 1, 1955, CCS 381, JCS Records.

the United States would employ “all means available” to defend the offshore islands.³⁵

Dulles, reversing course once more, accepted Radford’s basic argument. The communists, Dulles said, would probably doubt American resolve until American leaders decided to “shoot off a gun.” Very soon, the secretary added, the administration would have to “face up to the question whether its military program was or was not in fact designed to permit the use of atomic weapons.” If the administration continued to refrain from using them, and if it allowed popular sentiment against their use to grow, “we might wake up one day and discover that we were inhibited in the use of these weapons by a negative public opinion.” When that day came, Dulles concluded, “our entire military program would have to be revised.”³⁶

Eisenhower, characteristically, refused to decide the issue one way or the other, although in a news conference he responded to a query regarding nuclear weapons by saying that “in any combat where these things can be used on strictly military targets and for strictly military purposes, I see no reason why they shouldn’t be used, just exactly as you would use a bullet or anything else.” Fortunately for the administration, the People’s Republic of China let the United States off the hook. Beijing chose to deescalate, allowing a peaceful but inconclusive resolution of the crisis.³⁷

AGAIN, EISENHOWER HAD DUCKED THE FUNDAMENTAL QUESTION of American national-security policy—whether nuclear weapons were for use or simply for deterrence—and policy remained as muddled as ever. The Pentagon continued to agitate for a pro-nuclear posture even as it lobbied for a build-up of conventional forces. Early in 1956, Radford asserted that the cost cutting had gone far enough. “The free world situation,” the admiral declared, “is gradually deteriorating. Unless adequate steps are taken to change this trend, the United States will, in a span of a relatively short number of years, be placed in great jeopardy.” At the moment, the communists and the allies alike saw only “confusion and indecisiveness” on the part of American leaders. Radford repeated his “bloody nose” recommendation and called for an increase of at least \$3 billion annually for defense. Secretary of the Treasury Humphrey argued in just the opposite direction, complaining as late as 1957 that, for all the administration’s expressed concern regarding defense spending, nothing had “ever really been done to carry out this New Look policy.” The administration,

³⁵ Memorandum of discussion at 240th NSC meeting, March 10, 1955, *FRUS*, 1955–57, 2: 345–50; Radford to Wilson, March 27, 1955, 091 China, Radford files, JCS Records.

³⁶ Memorandum of discussion at 240th NSC meeting, March 10, 1955, *FRUS*, 1955–57, 2: 345–50. As throughout much of this period, Dulles was speaking with less than complete understanding of what the use of nuclear weapons would entail. According to Gerard Smith, the secretary of state’s consultant on atomic energy, Dulles underestimated the casualties an atomic defense of the offshore islands would produce. Upon learning that even “surgical” strikes opposite Jinmen would leave nearly 200,000 dead, his support for the use of tactical nuclear weapons diminished considerably. (Comments by Smith at Princeton University John Foster Dulles conference, February 26–28, 1988.)

³⁷ Quoted in Ambrose, *Eisenhower*, 2: 239.

Humphrey added, had been “led astray by scientists and by vested interests.” Secretary of Defense Wilson confessed to sabotage in the Pentagon, admitting that the army had been leaking anti-New Look information to the press. Eisenhower ritually reiterated his arguments about the need to bring spending into line with economic constraints, but he began to wonder whether the New Look, as originally planned, would ever become a reality. At a meeting of the National Security Council in August 1956, the president asked whether the manpower ceiling of 2.5 million was too low—which, of course, was exactly what the army was waiting to hear. At another session seven months later, Eisenhower admitted to a “sense of defeat” regarding his ability to balance economic and military demands.³⁸

Dulles shared the president’s discouragement. Shifting his position for a fourth time, Dulles told Eisenhower that the massive-retaliation policy “too much invoked mass nuclear attack in the event of any clash anywhere of U.S. with Soviet forces.” The secretary described the “vicious circle” that reliance on the threat of nuclear escalation entailed. “So long as the strategic concept contemplated this, our arsenal of weapons had to be adapted primarily to that purpose, and so long as our arsenal of weapons was adequate only for that kind of response, we were compelled to rely on that kind of response.” Dulles was not ready to abandon massive retaliation entirely, if only because a sudden shift would send the wrong signals to friends and enemies. “Our deterrent power might be somewhat weakened if it were known that we contemplated anything less than ‘massive retaliation.’” But the administration could not avoid the issue forever; American planners must learn to defend the United States by “means short of the wholesale obliteration of the Soviet Union.”

After hearing Dulles out, Eisenhower conceded that the administration had not thought completely through to the consequences of the New Look. “Our strategic concept,” the president said, “did not adequately take account of the possibilities of limited war.” Agreeing that something must be done, Eisenhower—typically—proposed another “high level review.”³⁹

By this time, Eisenhower had lost the initiative he had brought to the presidency; with scarcely half a term left, he held little terror for anyone, least of all the national-security bureaucracy, which could easily resist change for that much longer. Even Eisenhower’s supporters detected a slowing down and lamented the opportunities lost. In 1956, Dulles confided to former White House aide C. D. Jackson:

I wish there were some way to solve this problem of government. Arthur Vandenberg told me that the real reason that he was praying that Eisenhower would become President was that the Government of the United States had become so unwieldy, so practically unworkable, that Vandenberg thought it would require someone with the prestige and personality and drive, and above all, the popular mandate, of Eisenhower, to come in and reorganize and reinvigorate it—and of course that has not happened.

³⁸ Radford for JCS to Wilson, March 12, 1956, Staff Secretary Records; memoranda of discussions at 317th NSC meeting (March 28, 1957), 293d meeting (August 16, 1956), and 294th meeting (August 17, 1956), Eisenhower Papers.

³⁹ Dulles memorandum of conversation with Eisenhower, April 1, 1958, Dulles Papers.

Jackson, writing in his diary two years later, depicted the president as being "lovable as always, with many sense-making flashes . . . but not a national or political leader." Jackson continued, "He can sprint a few yards, but he tires quickly, and while he can become momentarily fascinated by individual pieces of the international jigsaw puzzle, he does not seem to be able to see what the picture would look like when all the pieces were put together." Jackson concluded his entry with the comment, "This impression amply confirmed by inside White House family when hair down."⁴⁰

Age and ill-health contributed to Eisenhower's difficulties in solving the problem of national security, especially after the president's heart attack in 1955; but much of the trouble stemmed from his style of decision making. The president once summarized his thoughts on crisis management: "I have so often been through these periods of strain that I have become accustomed to the fact that most of the calamities that we anticipate never really occur." Throughout his presidency, Eisenhower repeatedly put off choosing between alternative policies, hoping troubles would resolve themselves, as they sometimes did. But, whatever the merits of masterly inactivity in Indochina in 1954 or the Taiwan Strait in 1955 and 1958, Eisenhower's hands-off, procrastinating approach failed in matters of national-security planning. The continuing crisis of the nuclear age is peculiar in that it neither comes to a head nor goes away. By waiting for events to point the path to policy, and by thinking he could reserve judgment on the use or non-use of nuclear weapons until the last moment, Eisenhower virtually guaranteed that policy would drift down the course of least resistance.⁴¹

Eisenhower's troubles also followed from his habit of encouraging the parties to policy debates to discuss their views before full-dress meetings of the National Security Council, even after decisions ostensibly had been made. On the positive side, this habit allowed the president to examine all aspects of important questions. It also gave high officials a sense of participation in the policy process and a feeling of responsibility for the success of the policies the process produced. On the negative side, Eisenhower's failure to cut off debates in timely fashion encouraged dissenters to believe that if they kept talking they could gain another hearing or, at the least, safely delay implementation of the president's decisions.⁴²

Finally, the novelty of the nuclear problem contributed to Eisenhower's inability to find a solution. Like everyone else thinking about nuclear war at the time, Eisenhower was groping in the dark. During the early and middle 1950s, no one could predict with precision what a general conflict between the superpowers would entail. For most of his first term, despite the apocalyptic

⁴⁰ Dulles quoted in Jackson to Luce, April 16, 1956, C. D. Jackson Papers, Eisenhower Library; Jackson log, August 12, 1958, Jackson Papers. For an elaboration of this theme, see Brands, *Cold Warriors*, 185–200.

⁴¹ Diary entry for March 26, 1955, in Robert H. Ferrell, ed., *The Eisenhower Diaries* (New York, 1981), 296.

⁴² Eisenhower's use of the NSC was the subject of Anna K. Nelson, "Before the National Security Adviser: Did the NSC Matter?", a paper presented to the conference of the Society for Historians of American Foreign Relations at American University in Washington, D.C., June 10, 1988.

estimates of some of his advisers, Eisenhower operated on the assumption that a nuclear war would look much like the wars of his personal experience, only bigger. Through the beginning of 1956, administration officials charged with stockpiling strategic materials against the day of a general war with the Soviets planned on the conflict lasting three to five years. When skeptics in the administration challenged this scenario, Eisenhower reacted curtly. Some people, the president said at a January 1956 meeting of the National Security Council, contended that a nuclear war would be over in thirty to sixty days. This notion was "crazy." While admitting that a major war could wreak such destruction "that we might have ultimately to go back to bows and arrows," the president dismissed the idea that the conflict would end within eight weeks as being "about as specious as the idea of a race between himself and Secretary Humphrey to the moon."⁴³

Within the next several months, however, a new report by a special Panel on the Human Effects of Nuclear Weapons Development caused Eisenhower drastically to alter his thinking. According to this report, the United States could expect fifty million casualties in an initial exchange and its immediate aftermath, with spillover consequences that would critically impair if not eliminate altogether the ability of the United States to keep fighting. Eisenhower responded to the report with surprise bordering on shock. This was "the most serious problem which had ever faced the world," he said. "We should certainly revise our war plans."⁴⁴

Yet planning continued essentially as before. And, despite Eisenhower's altered understanding of the stakes involved in a nuclear conflict, the president continued to accept the plans. Only three months after the meeting just described, Eisenhower approved another National Security Council paper declaring that the United States should place primary reliance on nuclear weapons and should "consider them as conventional weapons from a military point of view." Notwithstanding his own comment that once a basic level of deterrence had been achieved, "we don't need to continue to build up," Eisenhower refused to restrain the weapons builders. On the contrary, when the Pentagon described the possibility of developing intermediate-range missiles, the president declared them to be of "paramount importance" to American security, and he pushed for their deployment at the "earliest practicable date." When intercontinental missiles became a technological likelihood, he accepted the view that for the Soviets to beat the United States to the deployment of these systems would have the "gravest repercussions on the national security and on

⁴³ See, for example, "Defense Mobilization Planning Assumptions," November 4, 1955, NSC series, OSANSA Records; minutes of 272d NSC meeting, January 12, 1956, *Declassified Documents*, 87/987; memorandum of discussion at 272d NSC meeting, January 12, 1956, NSC series, Eisenhower Papers.

⁴⁴ Report by Panel on Human Effects of Nuclear Weapons Development, November 21, 1956, NSC series, Eisenhower Papers; memorandum of discussion at 312th NSC meeting, February 7, 1957, *ibid.* On the panel's report, see Wm. F. Vandercook, "Making the Very Best of the Very Worst: The 'Human Effects of Nuclear Weapons' Report of 1956," *International Security*, 11 (Summer 1986): 184–95.

the cohesion of the free world,” and he authorized giving the ICBM program the “highest priority above all others.”⁴⁵

Eisenhower’s decision for all-out development of ICBMs—the last of his major decisions on weapons policy and arguably the most significant decision made by an American president during the nuclear era—followed logically, almost inevitably, from his earlier acceptance of nuclear weapons as the principal guarantor of American security. Yet, as logical as the decision was, and as reasonable as it seemed at the time, it soon revealed the deficiencies of Eisenhower’s basic approach to national security, and it gave rise in the longer run to consequences Eisenhower certainly did not intend when he adopted the New Look.

In October 1957, the Soviet Union launched a satellite into earth orbit. By itself, Sputnik did not alter the balance of strategic power between the United States and the Soviets, but it changed enormously perceptions of power. Although Americans at large did not know the details of the Eisenhower administration’s weapons-development program, they did know it was premised on American technological superiority; consequently, this evidence of Soviet superiority in the crucial field of rocketry came as a blow. The public peered worriedly at the sky; pundits wrung their pens; Congress demanded investigations into America’s perilous predicament. Charging Republican negligence in matters vital to American security, the Democrats raised a hue and cry that created nearly irresistible pressure for increased spending on defense. Eisenhower resisted, with no little success, but his very success proved counterproductive when the Democrats parlayed the issue of a “missile gap” into victory in the 1960 presidential contest, paving the way for the accelerated weapons build-up of the decade that followed.⁴⁶

The post-Sputnik uproar also illuminated more clearly than ever a trend that had been implicit in American defense policy since the adoption of the New Look. By basing American security on possession of the latest scientific and military technology, Eisenhower delivered enormous power over fundamental policy decisions to an elite of scientists, engineers, and defense bureaucrats whose activities the vast majority of Americans could not understand and, not understanding, could not knowledgeably control. Eisenhower recognized the problem, and he spoke to it directly in his farewell address. While Eisenhower’s warning against a “military-industrial complex” is often interpreted as the insight of a sage old general, it can also be read as an admission of defeat by a frustrated chief executive. Indeed, Eisenhower said he felt “a definite sense of disappointment.” And well he might have, for more than any administration

⁴⁵ Amendment to NSC 5707/7 in memorandum of discussion at 325th NSC meeting, May 27, 1957, NSC series, Eisenhower Papers; memoranda of discussions at 293d and 294th NSC meetings, August 16 and 17, 1956, NSC series, Eisenhower Papers; memorandum regarding 268th NSC meeting, December 1, 1955, OSANSA Records; record of actions at 317th NSC meeting, March 28, 1957, Staff Secretary Records; memorandum by Lay, September 16, 1955, *Declassified Documents*, 86/450.

⁴⁶ On the reaction to Sputnik, and Eisenhower’s reaction to the reaction, see Kinnard, *President Eisenhower and Strategy Management*, 66–122; and Ambrose, *Eisenhower*, 2: 423–30.

before or after, Eisenhower's promoted the growth of the military-industrial complex he decried.⁴⁷

Eisenhower's New Look (the massive retaliation) policy was counterproductive in other ways, as well. Militarily, its reliance on advanced weaponry accelerated an arms race that rendered the United States more, not less, threatened. Politically, its overblown rhetoric fostered an atmosphere in which reasoned consideration of defense issues became nearly impossible, as the aftermath of Sputnik demonstrated. Strategically, by failing to deal adequately with the challenge of peripheral and limited war that increasingly characterized international struggles, massive retaliation forced the development of a second track of American security policy. Bureaucracies being what they are, the new policy—"flexible response"—did not replace the old; it simply supplemented it. The country wound up with the worst of both worlds: the high risks of strategic warfare and the high costs of limited conflict. Eisenhower entered office determined to contain defense spending and reduce American vulnerability. For a time, he contained defense spending. But the inadequacy of his leadership, combined with the intractable problems he faced, rendered nearly inevitable major increases in weapons budgets and an America left more vulnerable than ever.

⁴⁷ *Public Papers of the Presidents: Dwight D. Eisenhower, 1960–1961* (Washington, D.C., 1961), 1038–40.

Review Article
**The Not-So-Dark and Bloody Ground:
New Works on the 1960s**

MAURICE ISSERMAN

Todd Gitlin, **The Sixties: Years of Hope, Days of Rage** (New York: Bantam Books, 1988).

James Miller, **"Democracy Is in the Streets": From Port Huron to the Siege of Chicago** (New York: Simon and Schuster, 1987).

David Farber, **Chicago '68** (Chicago: University of Chicago Press, 1988).

Ronald Fraser, *et al.*, **1968: A Student Generation in Revolt** (New York: Pantheon, 1988).

David Cate, **The Year of the Barricades: A Journey through 1968** (New York: Harper and Row, 1988).

IN THE 1988 PRESIDENTIAL CAMPAIGN, George Bush charged that the Democratic party suffered from a "split personality." Its rank-and-file members—whose votes Bush needed to win the election—were "made up of some of the best of America, the silent majority." Its leadership, he contended, was a different story: "[M]uch of it is a remnant of the 60's, the new left, those campus radicals grown old, the peace marchers and the nuclear freeze activists." As these comments suggest, the 1960s remained much on the minds of Americans in the 1980s, although the uses to which such memories could be put varied enormously. Some looked back to the decade of the 1960s and saw in it the explanation for many of the nation's current woes, others looked back to it for an inspirational model of social change, still others found in it a mother lode of nostalgia suitable for commercial exploitation.¹

The stark divisions in the popular assessment of the period have not carried over into the emerging historical treatment of the New Left, the movement that

For their careful reading and thoughtful criticisms of an earlier version of this article, I am grateful to Michael Kazin, Laura Frader, Marilyn Halter, and the editors of the *AHR* (not all of whom, I hasten to add, were persuaded by the views expressed herein).

¹ "Despite Vow to Be 'Gentler,' Bush Stays on Attack," *New York Times* (October 29, 1988): 10. Also see "Decade Shock," *Newsweek* (September 5, 1988): 14–29; "To '68 Leader, Columbia Is Still Lively," *New York Times* (March 28, 1987): 31; "A New Generation Shares the 60's," *New York Times* (July 11, 1987).

lies at the center of much of the controversy surrounding the 1960s. As evidenced by the books under review here, the history of the New Left has been told almost exclusively from the perspective of those who had direct experience in the movement and continue to share at least some of its original assumptions. Todd Gitlin was one of the early national leaders of Students for a Democratic Society (SDS), James Miller joined SDS in time to witness its collapse in the late 1960s, David Caute taught at the New Left-oriented Free School of New York and Anti-University of London, and Ronald Fraser served as an editor of the British journal *New Left Review*. David Farber is the exception, too young to have had any direct political involvement in the 1960s, but even he was intrigued enough in August 1968 as an eleven-year-old Chicago native to wander over to Grant Park "to see the hippies," before being warned off by the police. The portrait of the New Left that emerges from these books is one of a movement that, though not without its flaws, challenged a generation of Americans to confront the worst shortcomings of their own society. They see the New Left's radicalism as intensely indigenous; indeed, for the first time in the history of the Left in the United States, American radicals had more influence on their European counterparts than Europeans did on the Americans. These authors treat the New Left as a serious movement that broke important ground politically and culturally, even though it fell short of realizing the inflated dreams of the era. (Caute, as will be shown, is a partial dissenter from this consensus, arguing that the American New Left did not attain the standards of bold action and incisive analysis he sees having been set by part of the European New Left.) Judging from the near unanimity that prevails among these authors, what one might have expected to be the "Dark and Bloody ground" of the historiography on the 1960s remains thus far remarkably uncontested terrain.²

AMONG THE MOST COMMON ACCUSATIONS lodged against the New Left by its detractors—in the 1960s and ever since—is that it was oblivious of history. This is, at best, an oversimplification. The movement had a tradition of historical self-scrutiny implicit in its name: to speak of a "new" Left assumed that there was another, older Left against which the movement could and should be measured.

² Peter Collier and David Horowitz's *Destructive Generation: Second Thoughts about the '60s* (New York, 1989) is a collection of memoirs, polemics, and historical sketches from two journalists whose publishing venue and ideological perspective has shifted over the years from *Ramparts* to *Commentary*. On the whole, the book tries too hard to be the *Witness* of the 1980s, but it has some insights to offer on such topics as the Weather Underground and the Black Panthers. Horowitz and Collier's accounts of their personal disillusionment with the New Left are reprinted, with similarly repentant fare from ten other authors, in John H. Bunzel, ed., *Political Passages: Journeys of Change through Two Decades, 1968–1988* (New York, 1988). The few available scholarly books on the New Left written from a conservative perspective have come not from historians but from sociologists like Cyril Levitt, *Children of Privilege: Student Revolt in the Sixties* (Toronto, 1984), or political scientists like Stanley Rothman and S. Robert Lichter, *Roots of Radicalism: Jews, Christians and the New Left* (New York, 1982). Both books are reviewed critically and perceptively by Wini Breines, "The Sixties Again," *Theory and Society*, 14 (1985): 511–23. The most thoughtful academic treatment of the New Left from a conservative viewpoint can be found in another book by a political scientist, Guenter Lewy's *Peace and Revolution: The Moral Crisis of American Pacifism* (Grand Rapids, Mich., 1988).

Strongest in its early days, the movement's sense of historical perspective waned but did not completely disappear, even in its apocalyptic final days. Elinor Langer, a Swarthmore College undergraduate from 1957–1961 who went on to be a radical activist in the 1960s, contributed one of the most interesting memoirs of the era in her essay "Notes for Next Time." At Swarthmore, she remembers,

Most of my friends and I were drawn to studying history, and for a lot of reasons we felt we understood a great deal about revolution and change, about how history happens . . . We identified with every revolution we could discover, and every revolutionary, those in the papers . . . as well as in the past. Knowing what we "knew" about revolutions in history, it was difficult to understand how politicians could be so stupid as to oppose them in the present, or why they would not just sit back, like our teachers, observe, and wish the people well.³

More than most, the decade that followed Langer's graduation offered many Americans a sense of living in the midst of history in the making. These were years in which the radical news director of a popular FM radio station in Berkeley, California, would sign off his broadcasts with the signature closing, "If you don't like the news, go and out and make some of your own."⁴ Making "news" and remembering the past were not necessarily compatible activities, as became apparent when the movement broadened its base in mid-decade well beyond the intensely intellectual core of supporters it initially attracted on campuses such as Swarthmore, Oberlin, Michigan, and Berkeley. Jack Newfield, an early member of SDS, published *A Prophetic Minority* in 1966, the first serious examination of the historical roots of the then-blossoming New Left. Despite his sympathies for the movement, Newfield reported an "appalling anti-intellectualism" among younger members of SDS, none of whom had ever read Rosa Luxemburg, Max Weber, Eduard Bernstein, John Dewey, Peter Kropotkin, or John Stuart Mill.⁵ Whatever their deficiencies in political theory and extended historical perspective, New Leftists nevertheless self-consciously thought of themselves as living within and making history. The very slogans of the movement recorded a fascination with contemporary historical milestones: "From Protest to Resistance," was the phrase the radical wing of the antiwar movement coined for its embrace of direct-action tactics in the fall of 1967; "From Moral Outrage to Radical Vision," was how an SDS recruiting pamphlet, published in 1968, summed up the events of the first eight years of the organization's history.⁶ New Leftists looked for confirmation of their own

³ Elinor Langer, "Notes for Next Time, A Memoir of the 1960s," *Working Papers*, 1 (Fall 1973): 5.

⁴ Newscaster Roland Young is quoted in Mitchell Goodman, ed., *The Movement toward a New America* (New York, 1970), 420.

⁵ Jack Newfield, *A Prophetic Minority* (New York, 1966), 87.

⁶ "SDS, An Introduction," SDS National Office, 1968 (copy in author's collection). Joseph Conlin, in his accurately subtitled *The Troubles: A Jaundiced Glance Back at the Movement of the Sixties* (New York, 1982), argued that "woeful ignorance of history was a predictable consequence of Movement antihistoricalism, but even here the militants exceeded all expectations . . . The New Left did not even know its own history beyond a day or two of the past"; 314. In fact, for a movement with such a short history, New Left publications seem filled with an inordinate amount of historical self-reflection. See, for example, James P. O'Brien's series "The New Left," in *Radical America* (May–June 1968): 1–25; (September–October 1968): 1–22; and (November–December 1968):

historical importance in Newfield's influential book, in journals like *Radical America*, and in no less than four historically minded documentary collections published by 1970. In the preface to one such collection, Staughton Lynd predicted that, when the history of the New Left was written, it would amount to a "'guerrilla history' as different from conventional history as guerrilla theater is different from the theater of Shakespeare"—which, in the context of the times, was meant as a compliment to would-be historian-guerrillas.⁷ New Leftists also left behind a thick archival record of their own doings. The last above-ground act of the Weathermen SDS faction in early 1970 was to turn over the papers in the SDS National Office to the Wisconsin State Historical Society—although their motivation undoubtedly represented less a concern for future scholars of the movement than for the quick \$300 payment the society had to offer.⁸

A nonacademic historian, Kirkpatrick Sale, was the earliest explorer of the Wisconsin archives. He also conducted the first extensive interviews with SDS veterans after the 1960s. The resulting book, titled *SDS* and published in 1973, set in place the interpretive guidelines that most subsequent chroniclers of the New Left have followed. Sale described the influence of the civil rights movement on SDS in the early 1960s; the New Left's disaffection for liberalism in the mid-1960s; the influx of new recruits into SDS in the aftermath of the Vietnam War's escalation, and the resultant rise of a "prairie power" politics within the organization oriented toward direct action and counterculture; the capitulation of the SDS "old guard" to these new trends; the destructive influence of the Maoist Progressive Labor party as it infiltrated its members into SDS; and the growing infatuation of SDS's National Office with competing notions of "vanguard party" politics.

In accounting for the downfall of SDS, Sale placed his emphasis on faulty political choices made by individual leaders; better political choices, he suggested, might have saved the organization. The closing chapters of his book are suffused with a sense of lost opportunities. In describing the embrace of a frenetic Marxism-Leninism by the last generation of SDS leadership, Sale quoted "old guard" member Steve Weissman: "We should have done more to stop them when they took over in 1967 . . . It was just a decision by a few people, the people around the National Office." Sale commented, with evident regret: "But the old guard did not move."⁹ Sale's interpretation suffers in places from a lack of perspective and perhaps a too-recent involvement with the enthusiasms he sought to analyze (as in the unfounded claim that "maybe as much as a fifth of the population" of the United States sympathized with the political violence of

28–43. For a brief discussion of the New Left's historical self-consciousness, see Maurice Isserman, "1968 and the American New Left," *Socialist Review*, 18 (October–December 1988): 95–96.

⁷ Staughton Lynd's comments preface Priscilla Long, ed., *The New Left: A Collection of Essays* (Boston, 1967), 2. Also see Paul Jacobs and Saul Landau, *The New Radicals: A Report with Documents* (New York, 1966); Mitchell Cohen and Dennis Hale, eds., *The New Student Left* (Boston, 1967); and Massimo Teodori, *The New Left: A Documentary History* (New York, 1969).

⁸ Kirkpatrick Sale, *SDS* (New York, 1973), 647.

⁹ Sale, *SDS*, 395.

the New Left's last days).¹⁰ But, as a basic organizational history, *SDS* is difficult to fault. An anonymous FBI reviewer recommended it—even if he was confused about historical genres—in an internal Bureau memorandum in 1977 as “probably the best single biography of the SDS ever written.”¹¹

WITH THE PASSAGE OF ANOTHER DECADE AND A HALF, scholars have begun to return to the territory first explored in *SDS*.¹² Todd Gitlin's and James Miller's recent books add new dimensions to the history of the movement: Gitlin emphasizes the cultural influences that shaped the New Left's politics, while Miller builds his study on an analysis of the intellectual evolution of a group of early SDS activists. Both bring something to the story largely missing in Sale's version: a sense of causality that borders on predestination. If they are right, no last-minute change of strategy or leadership would have sufficed to save the New Left from problems that were present from the beginning. Acting with the best of intentions, SDS founders set in motion a political dynamic that would in the end destroy the movement they had invested so much of their hopes, energies, and identities in.

Gitlin opens *The Sixties* with a list of “unavoidable dilemmas” with which the New Left had to contend. In the absence of a credible adult Left, the New Left felt compelled to fill the vacuum and, in doing so, “oscillated between narcissism . . . and self-disparagement,” constantly on the lookout for the true historical agent of change (the working class, the Third World, “revolutionary youth”). Beginning their active political involvement during the momentary ascendancy of liberalism in Washington, New Leftists saw all too little cause to worry about the consequences of provoking a reaction from a not-so-dormant Right. Surrounded from early childhood by the products and assumptions of the mass-culture industries, New Leftists accepted and even came to embrace the media's imposition of its own agenda on movement strategy, organization, and public presentation. Perhaps the key dilemma in the list Gitlin presents and the source of much of the movement's initial strength as well as its later troubles was the adoption by the New Left of a stance intended to avoid the mistakes of failed radical predecessors. The New Left “wanted to be both strategic and expressive, political and cultural: to change the world (end the war, win civil rights) while

¹⁰ Sale, *SDS*, 635.

¹¹ See the FBI report “Weather Underground Organization,” June 21, 1977, E, FBI Weathermen Files No. 100-439048-3816, Washington, D.C.

¹² Other histories of the New Left published in the decade after the movement's collapse include Irwin Unger, *The Movement: A History of the American New Left, 1959-1972* (New York, 1974); Thomas Powers, *Diana: The Making of a Terrorist* (Boston, 1971); Milton Viorst, *Fire in the Streets: America in the 1960s* (New York, 1979); Dick Cluster, ed., *They Should Have Served That Cup of Coffee: 7 Radicals Remember the 60s* (Boston, 1979); Wini Breines, *Community and Organization in the New Left, 1962-1968: The Great Refusal* (New York, 1982). More broadly focused books that devote one or more chapters to the New Left include William L. O'Neill, *Coming Apart, An Informal History of America in the 1960's* (New York, 1971); Thomas Powers, *Vietnam: The War at Home* (New York, 1973); and Allen J. Matusow, *The Unraveling of America: A History of Liberalism in the 1960s* (New York, 1984).

freeing life in the here and now.”¹³ Movement activists found themselves increasingly incapable of distinguishing between political choices that merely provided a gratifying sense of “commitment” and those that actually made sense in terms of American political realities.

An arms control activist as an undergraduate at Harvard, Gitlin was elected national president of SDS in 1963 at the age of twenty and spent the mid-1960s as a community organizer in Chicago. He was on hand for the epic street confrontations in Chicago at the Democratic National Convention in 1968 and in Berkeley during the People’s Park disorders of 1969. Drawing on these experiences, he effectively straddles the divide between history and memoir in *The Sixties*. A keen observer and capable writer, Gitlin captures the shifting nuances of mood and dynamic within the New Left from year to year but writes most knowingly of “the old New Left, the pre-Vietnam New Left,” destined to be “the small motor that later turned the larger motor of the mass student movement of the late Sixties.”¹⁴

Even though the “old New Left” members would be out of leadership and out of favor by the time SDS embraced the cult of the vanguard party, Gitlin does not absolve them (or himself) of responsibility for what followed; instead, he argues that “this minority created a tradition—a culture, a style, an approach to society, a set of tactics—that played itself out in the movement’s subsequent history.”¹⁵ The sources for that culture, style, approach, and set of tactics Gitlin finds in the “Zeitgeist” that began taking form in the late 1950s—a word that, in Gitlin’s half-ironic usage, suggests an amalgam of high and low, popular and political, culture that almost but not quite attained its own animating intelligence. The “Zeitgeist” on the eve of the 1960s represented a collection of influences, ranging from *I. F. Stone’s Weekly* to *Mad Magazine*, from the Beats to Bill Haley and the Comets, and from John Kenneth Galbraith to C. Wright Mills. Only a small minority of America’s young were attuned to the entire list, but it was a minority broad enough to include someone like Gitlin, with his liberal, Jewish, New York upbringing, and Tom Hayden, whose childhood was spent in a lower-middle-class Catholic neighborhood of Royal Oak, Michigan.¹⁶

As the 1950s gave way to the 1960s, Gitlin, Hayden, and thousands of others like them came to share a sense of generational identity, alienation, and mission. This connection took shape in both “subterranean channels,” such as those provided by the Beat writers and poets, and, within the confines of the dominant culture, in such Hollywood offerings as *Rebel without a Cause*. When that teen epic’s star, James Dean, died in a car crash a few days before the film’s release, he was destined for immortality as an icon of youthful rebellion and youthful self-preoccupation. Gitlin notes: “This sense of a fatal connection to young

¹³ Todd Gitlin, *The Sixties: Years of Hope, Days of Rage* (New York, 1987), 5–6. In an earlier book on the New Left and the media, Gitlin argued that “the internal frailties that were to undo [SDS] were already built in at the moment of its greatest growth and vigor”; Todd Gitlin, *The Whole World Is Watching: Mass Media in the Making and the Unmaking of the New Left* (Berkeley, Calif., 1980), 31.

¹⁴ Gitlin, *The Sixties*, 26.

¹⁵ Gitlin, *The Sixties*, 26.

¹⁶ Gitlin, *The Sixties*, 11–77; Tom Hayden, *Reunion: A Memoir* (New York, 1988), 12–21.

martyrs, of death as the final refutation of plenitude, ran strong through all the phases of Sixties culture.”¹⁷

Cultural disaffection merged with political insurgency in the early 1960s. In the course of wresting long-denied and dearly paid-for rights for southern blacks, the civil rights movement set a standard of heroic individual commitment (“putting your body on the line”) while offering a glimpse of a new way of living (the “beloved community”). Although the civil rights movement is not a major focus of Gitlin’s book, it is still clear from his account that there would have been no “Sixties” without it: the Free Speech movement, pacifism, feminism, the counterculture, and the New Politics within and without the Democratic party were all, in critical ways, given inspiration and shape by the struggle in the South. The young black organizers who risked their lives in the Deep South (along with some white allies), were affected in ways both superficial and profound by their experiences: “What began as strategy became identity. SNCC [Student Nonviolent Coordinating Committee] organizers, mostly city-bred, picked up the back-country look of Georgia and Mississippi: denim jackets, blue work shirts, bib overalls.”¹⁸ Styles of dress, patterns of speech, an intensely moralistic approach to politics, and a growing suspicion of the good intentions of white liberals spread from SNCC to SDS, from the back country of Georgia and Mississippi to campuses like Ann Arbor and Berkeley.

For reasons left unclear, Gitlin pays little attention to the mid-1960s. The “years of hope,” in his version of the decade, had already begun to erode by the autumn of 1964, although it might be argued that, with the great civil rights victory at Selma, Alabama, still to come in 1965, and the height of countercultural utopianism not attained until 1967, this is an instance in which *The Sixties* is perhaps more reliable as memoir than history. The dramatic climax of the book is reached with the onset of the “days of rage,” roughly, 1968–1970. It was a time when, in Gitlin’s treatment, the good ideas and impulses of the early movement gave way to bad ideas and impulses, but in some ways the “good” was still embodied in and even most fully realized in the “bad.” Whatever chance the New Left might have had to remain true to its early, hopeful, nonviolent, and reasoned commitments was destroyed by the war in Vietnam. With the escalation of the war, the logic of those early “unavoidable dilemmas” carried the New Left steadily on toward its destruction. The New Left’s “Third World turn” represented the triumph within the movement of “expressive politics,” marked by the appearance of National Liberation Front flags at antiwar protests. Antiwar militants were deaf to the argument that the public embrace of such inflammatory symbols would only harm their cause:

Desperate for moral companionship—America having forfeited our love—a part of ourselves looked with respect, even awe, even love, on an ideal version of ourselves who we thought existed—*had* to exist out there in the hot climates. We needed to feel that

¹⁷ Gitlin, *The Sixties*, 32–34.

¹⁸ Gitlin, *The Sixties*, 163.

someone, somewhere in the world, was fighting the good fight and winning . . . The issue became *how we felt* more than *what would end the war*.¹⁹

Many in the New Left came to despair of ever ending the war through peaceful protest; by the late 1960s, Gitlin writes, violence was “endlessly talked about, feared, skirted, flirted with . . . Violence organized the movement’s fantasy life.” Gitlin’s associate from the early leadership of SDS, Tom Hayden, “embodied these moods,” and he worked out a strategic rationale for militant, disruptive street confrontations: if the antiwar movement was able to polarize the nation over the issue of the war, and “arouse the sleeping dogs on the Right,” in Hayden’s phrase, the liberal elite running the country would, out of self-interest, be forced to step in to end the source of discontent. But Gitlin believes that something deeper than “strategic” concerns was at work: “My guess is that most of the demonstrators who went to Chicago were driven, like Hayden, to test themselves.”²⁰ The spirit of James Dean was much in evidence in the battles of Lincoln and Grant parks.

Gitlin is critical of later leaders of SDS for their sectarianism and nihilism. But such criticisms are almost beside the point given the logic of his argument: “The harder I looked at the movement’s history, the less I saw simple error, the more I saw the Weathermen embodying the worst of both poles of the movement’s long-standing built-in dilemma. Their guilty Third World-ism was a caricature of the ‘politics-for-others’ stance, their arrogance an extension of ‘politics-for-selves’ . . . [T]he more predestined the Weathermen and the SDS crack-up seemed, the more depressed I felt.”²¹

Predestination is also a theme of James Miller’s *“Democracy Is in the Streets.”* Miller complements Gitlin’s focus on cultural issues with his own effort to uncover the “intellectual foundations of the New Left.” The key text in his interpretation is the SDS Port Huron Statement (reprinted in full as an appendix to the book), which Miller believes “remains a document of considerable intrinsic interest and intellectual substance . . . the New Left at its most thoughtful and plainspoken.”²² The founding manifesto of SDS called on young people to transform American politics through the creation of new forms of decentralized democracy that would supplement existing representative institutions and draw people directly into political decision making—hence “participatory democracy,” a radical vision that broke with both the dominant “pluralist” assumptions of American political theory and received left-wing dogma.

Miller infuses his history of ideas with a sense of drama, movement, and personality, by means of a collective biography of a small group of key actors among those who had gathered at Port Huron in 1962 to debate the future of

¹⁹ Gitlin, *The Sixties*, 262–63. Two recent studies suggest that the movement against the war in Vietnam accomplished much more than it dreamed of at the time. See Nancy Zaroulis and Gerald Sullivan, *Who Spoke Up? American Protest against the War in Vietnam, 1963–1975* (New York, 1984); and Melvin Small, *Johnson, Nixon, and the Doves* (New Brunswick, N.J., 1988).

²⁰ Gitlin, *The Sixties*, 289–91, 316–18.

²¹ Gitlin, *The Sixties*, 397.

²² James Miller, *“Democracy Is in the Streets”: From Port Huron to the Siege of Chicago* (New York, 1987), 14.

the movement. The representative figure is, again and inevitably, Tom Hayden, author of the first draft of the Port Huron Statement and the SDS's best-known spokesman. In Hayden's writings of the early 1960s, Miller finds "a constant tension between civic republicanism on the one hand and existentialism on the other: When he follows [C. Wright] Mills and his own teacher Arnold Kaufman, he depicts a world of orderly face-to-face discussion among responsible citizens: when he follows [Albert] Camus and his own enthusiasm for the daring politics of direct action, he depicts a world of clashing wills and romantic heroes, mastering fate through the hard assertion of personality."²³

By following Hayden and a half-dozen or so other Port Huron veterans through the next few years, Miller traces the politically promising notion of "participatory democracy" as it turned into a shibboleth for the movement, increasingly indistinguishable from the quest for personal "authenticity." Hungering for a sense of direct participation in social change that could bring with it the satisfactions of community that SNCC organizers in the South were experiencing, the founding generation of SDS set off virtually en masse in the summer of 1964 for an experiment in "building an interracial movement of the poor" known as the Economic Research and Action Project (ERAP). In terms of numbers involved, it was an insignificant venture, with about five hundred members of SDS involved in the summers of 1964 and 1965. Its practical results were also limited: despite some local successes, no "interracial movement of the poor" emerged. But in terms of the impact the project had on the leadership of SDS, ERAP fully deserves the attention given it in Miller's account.

In a setting of voluntary asceticism, where peanut butter was the main staple of the organizers' diet, the members of ERAP settled into a more-or-less constant meeting, when they were not out knocking on doors or handing out leaflets. Every decision proved a topic for agonized discussion. ERAP members in Cleveland once devoted twenty-four hours to a discussion of whether or not they would permit themselves to take a day off from organizing to go to the beach. For some participants, ERAP provided a much-appreciated opportunity for personal development; women, in particular, blossomed as organizers. But, in other ways, the experiment with community organizing worked to the detriment of the broader movement. It took SDS's most experienced leaders away from the campuses at precisely the moment that the campus New Left began to grow in strength, and it helped discredit both the style and substance of reform politics. "Set beside the intensity of [the ERAP] experience, the routines of ordinary electoral politics, the mechanics of registering voters and counting votes, the cutting of deals and trimming of sails, seemed like a pale substitute, a poor excuse—an imitation of political freedom, and not the real thing."²⁴

When "prairie power" recruits began pouring into the New Left, older leaders of SDS were left disarmed in the face of the ultra-decentralist and countercultural impulses the newcomers brought with them. "Direct action" was the preferred form of political activity because it involved a personal affirmation of

²³ Miller, *"Democracy Is in the Streets,"* 145–46.

²⁴ Miller, *"Democracy Is in the Streets,"* 216.

political commitment, and it could be initiated and controlled on the local level. Miller's account of political evolution in SDS during the later 1960s is steeped in irony. Despite the best intentions of their initiators, ideas that called for expanding democracy eventually turned into their opposites. By the late 1960s, the notion took root within SDS that participatory democracy should replace, rather than enrich, existing forms of representative democracy, opening the way for the triumph of varieties of Marxism-Leninism antithetical to the original democratic and decentralist vision of SDS.

There are some problems of emphasis within *"Democracy Is in the Streets."* Focusing as closely as he does on a few major SDS figures of the early years, Miller gives minimal attention to some of the most important events in the emergence of the New Left. The anti-nuclear testing protests of the early 1960s, the Mississippi Summer project, the Berkeley Free Speech Movement, and the antiwar teach-ins are ignored or mentioned only in passing. The crucial role of the civil rights movement in forming the consciousness of the new generation of white activists can only be inferred from Miller's account. And, although it may have seemed otherwise to activists at the time, Tom Hayden could not show up everywhere; when he appeared, his role was sometimes quite marginal. But, for those events in which Hayden and other Port Huron alumni did play important roles, *"Democracy Is in the Streets"* is effective as both a descriptive and analytic guide. After the Newark riots of 1967, Hayden left ERAP and devoted himself to antiwar organizing. He was one of the main organizers for the protests at the Democratic National Convention in August 1968, where demonstrators chanted, "The whole world is watching!" while police savaged them in front of the television cameras. But what, Miller asks, was the world being invited to watch? "Was a crowd helplessly chanting in the midst of a police riot the image of participatory democracy? Was street fighting the seed of 'a people's movement'? Was this really what a generation's moral revulsion against the Vietnam War and idealistic quest for a 'democracy of individual participation' had come down to?"²⁵

The history of the New Left, in Miller's view, was that of a failed attempt to reconcile its two intermingled founding traditions of civic republicanism and existentialism, with the existentialism ultimately triumphing. Rather than taking their own earliest and best ideas seriously, Miller argues, SDS members found themselves swept along in a flood tide of unexamined impulse and emotion that, by decade's end, had virtually wiped out all memory of the hopeful beginnings of the politics of participatory democracy.

IT HAS BEEN A MATTER OF CONCERN for some reviewers of *The Sixties* and *"Democracy Is in the Streets"* that these works focus largely on a national organization like SDS, when so much of the political energy of the decade arose spontaneously from below. SDS, although the largest group on the left, was by

²⁵ Miller, *"Democracy Is in the Streets,"* 305.

no means the only show in town: the histories of the Berkeley Free Speech Movement (and, for that matter, much of California's New Left), draft resistance, feminism, gay and lesbian activism, and a myriad of more locally organized groups and concerns get short shrift in SDS-centric accounts like Gitlin's and Miller's. The history of the 1960s, the critics argue, must move beyond the boundaries of organizational history and leadership biography toward something like the "history from the bottom up" that an earlier generation of New Left historians demanded in other fields.²⁶ To do so, future historians of SDS might shift their focus from the activities of national leaders to those of the members of the hundreds of local chapters on campuses across the nation. Extensive and as yet virtually untapped resources are available for such a project, including SDS chapter correspondence in the Wisconsin Historical Society collection, college archives, FBI reports, and local underground papers. They also could interview more than the "usual suspects" in the network of SDS veterans.²⁷

To see one of the new perspectives opened up by a focus on local chapters, consider the question of the origins of the contemporary women's movement. The most influential interpretation of feminism in the 1960s can be found in Sara Evans's book *Personal Politics* (1979). Her thesis, reiterated in *Free Spaces*, the book she recently co-authored with Harry Boyte, is that involvement in the New Left both gave rise to and stifled the quest of young women for self-respect and equal rights. Participation in the southern civil rights movement or in ERAP's community-organizing projects "offered women opportunities to develop leadership skills, political analysis and role models." But the campus Left and the internal politics of SDS proved to be bastions of male chauvinism: "When women raised the question of their equality within the movement, they met a combination of indifference, ridicule, and anger. Women were hooted at, threatened with rape, accused of divisiveness and betrayal."²⁸ While the emergence of the new feminism was an exhilarating experience for many women, it also proved a bruising one for both women and men in the New Left, cutting as closely as it did to matters of the heart, domestic relations, and individual self-image. Rayna Rapp recalled of her male comrades at the University of Michigan that "they had

²⁶ See, for example, Paul Berman, "Don't Follow Leaders," *New Republic* (August 10 and 17, 1987): 28; Wini Breines, "Whose New Left?" *Journal of American History*, 75 (September 1988): 545; and Richard Flacks, "What Happened to the New Left," *Socialist Review*, 19 (January-March 1989): 98-99. On the reasons for the relative weakness of SDS at Berkeley, see W. J. Rorabaugh, *Berkeley at War: The 1960s* (New York, 1989), 90.

²⁷ The existing published accounts of the activities of SDS chapters date from the late 1960s and early 1970s, including Jerry L. Avorn, et al., *Up against the Ivy Wall: A History of the Columbia Crisis* (New York, 1968); William H. Orrick, Jr., *Shut it Down! A College in Crisis: San Francisco State College, October 1968-April 1969* (Washington, D.C., 1969); and Lawrence F. Eichel, et al., *The Harvard Strike* (Boston, 1970). My own thinking on the advantages of a chapter-focused approach was influenced by reading two recent and precocious unpublished theses by Harvard University seniors: Steven Daniel Pizer, "'Defeat False Unity!': A Study of Sectarianism in the New Left as Exemplified by SDS and the Harvard Strike" (Harvard University, 1985); and Douglass C. Rossinow, "The New Left Persuasion: Community and Individualism in Students for a Democratic Society, 1960-1970" (Harvard University, 1988). Pizer and Rossinow are cited with permission of the authors.

²⁸ Sara Evans and Harry C. Boyte, *Free Spaces: The Sources of Democratic Change in America* (New York, 1986), 104; and Sara Evans, *Personal Politics: The Origins of Women's Liberation in the Civil Rights Movement and the New Left* (New York, 1979), *passim*.

all this empathy for the Vietnamese, and for black Americans, but they didn't have much empathy for the women in their lives; not the women they slept with, not the women they shared office space with, not the women they fought at demonstrations with. So our first anger and anguish and fury was directed against the men of the left."²⁹

Testimony available from other women and some of the men who were in SDS leaves no doubt about the genuineness and the legitimacy of that "anger and anguish and fury." In the cacophony of radicalism in the late 1960s, voices had to be raised in order to gain attention. Because the feminist challenge came along when it did—at a moment when, for other reasons, the New Left was already moving into the wildest and most self-destructive phase of its political trajectory—the stakes seemed higher, the rhetoric was necessarily sharper, the sense of betrayal more pronounced. But with the passage of time—and viewed in contrast with the experience of earlier generations of feminists in the abolitionist, trade union, and socialist movements—what seems most striking to me about the New Left's encounter with the feminist challenge is not blind and unyielding male resistance but just the opposite: the triumph within the New Left of women's concerns.³⁰ When the issue was joined in SDS beginning about 1967, it only took one or two hard blows, historically speaking, to bring the edifice of male supremacy tumbling down. This is not to claim that every male chauvinist had reformed or departed the premises of the New Left by the start of the 1970s. But, at least by then, charges of "sexism" within the movement were being accorded a privileged attention similar to that previously reserved for charges of "racism."

Evans, noting the presence of many "red-diaper babies" (children of Old Left activists) within the New Left, suggests that "such radical families often provided the model of a politically active mother." Gitlin, who accepts the main outlines of Evans's indictment against SDS, echoes her suggestion that the emergence of feminism within the New Left was at least in part a reflection of the backgrounds of women activists. They grew up in "relatively egalitarian families" in which "mothers usually worked outside the home [and] parents took girls' intellects as seriously as boys."³¹ While the evidence for this argument remains impressionistic, it sounds right. But was it not exactly the same kind of families that

²⁹ Quoted in Ronald Fraser, *et al.*, *1968: A Student Generation in Revolt* (New York, 1988), 301.

³⁰ While it had little to do with the ultimate fate of the movement, in the charged atmosphere of the late 1960s New Left males were not given much opportunity to prove their good intentions. In 1915, when the American Socialist party decided to abolish its Woman's National Committee, Socialist leader Kate Richards O'Hare was moved to complain, "Scratch a scientific male Socialist and you will find an ordinary he-man. And that he-man will resent as bitterly and fight as unscrupulously the invasion of women into his domain as possible." That was more than thirty years after the publication of August Bebel's *Die Frau und der Sozialismus* had committed the world's socialists, in theory, to the idea of women's equality. Mari Jo Buhle, *Women and American Socialism, 1870-1920* (Urbana, Ill., 1981), 308. In 1967, thanks to an infamous anonymous editorial decision at the SDS *New Left Notes*, the published report of an SDS workshop on women's liberation was accompanied by a cartoon of a placard-bearing young woman whose polka dot panties are visible beneath her matching minidress. Sara Evans declared of the incident, which occurred scarcely two years after feminist issues were first raised in the white New Left, that "SDS had blown its last chance"; Evans, *Personal Politics*, 192.

³¹ Evans, *Personal Politics*, 122; Gitlin, *The Sixties*, 365.

produced the New Left's male leadership? And if so, why were they all—or even, were they all—as resistant to the concerns of the women's movement as Evans suggests?

In Evans's interpretation, the feminist message is brought to SDS from “free spaces” outside the organization—the first and key document being a “kind of memo” written by SNCC staffers Casey Hayden and Mary King and circulated among women in the movement a few weeks before the SDS National Council meeting at Champaign-Urbana in December 1965. (It was at that meeting that, for the first time, a women's caucus gathered to discuss common grievances.)³² Evans deserves credit for drawing attention to this memo, which had gone unmentioned in Sale's *SDS* and was even left out of Robin Morgan's anthology *Sisterhood Is Powerful* (1970). There was another memo that circulated that fall concerning the status of women in the movement, this one overlooked by all historians who have treated the issue and the period—a memo generated from within SDS and written, interestingly enough, by a male SDS leader. Donald McKelvey, assistant national secretary of SDS from 1962 through 1964, complained about the dismal level of personal relations existing in the National Office. Among the examples he cited was the toleration of a “sexual caste system” within SDS: “[F]emales are taught from their earliest years that they are inherently inferior, incapable of being competent except in limited, pretty rigidly defined areas . . . We're not personally sensitive to this problem as it is felt by girls in SDS; and it's not the kind of 'issue' which we feel is important to discuss as we organize.”³³ McKelvey's challenge was an indictment of SDS; considering its source, it also raises some doubts about the validity of the “free space” argument. McKelvey came to his insights from within, not from without, the organization, and, obviously, he was not himself the victim of the sexism he decried. Of course, the importance of his memo should not be exaggerated. It attracted little attention at the time, perhaps because he was not regarded as part of the inner circle of SDS leaders. He may well have been an aberration from the male SDS norm in his concerns—but, in 1965, Mary King and Casey Hayden were themselves aberrations from the female norm.

Judging from Gitlin's account, the SDS National Office, with its pressure-cooker atmosphere, and the premium placed within it on forceful public speaking as the mark of leadership, was not a hospitable place for the recognition of the concerns raised in the memos by Hayden and King or that by McKelvey; judging from the subsequent testimony of many female members of SDS, like that of Rayna Rapp, that lack of concern for women's issues also was the case in such flagship chapters of SDS as the one in Ann Arbor. There are indications, however, that within at least some chapters a different atmosphere prevailed. Writing in SDS's internal *Bulletin* in 1965, Judy Pardun contrasted her unhappy experience working in the National Office with her memories of the

³² Evans, *Personal Politics*, 161. Also see Mary King, *Freedom Song, A Personal Story of the 1960s Civil Rights Movement* (New York, 1987), 459, 467.

³³ McKelvey's memo, which can be found on Reel 21 of the SDS papers, is undated, but McKelvey recalls having written it for the Champaign-Urbana meeting. Author's interview with Don McKelvey, January 24, 1989.

“real community” she felt had prevailed in her home chapter in Austin: “[W]e treated each other with affection and respect . . . we had a real alternative to the present American society . . . Without verbalizing it, somehow we all felt it was necessary to treat people as individuals, not things; that in order to achieve our goals one had to, as much as possible, live them as we were fighting for them.”³⁴

The language Pardun employed, though not self-consciously feminist in inspiration, still anticipates values and aspirations of the women’s movement. As Don McKelvey had argued, SDS in the early to mid-1960s lacked a conception of politics inclusive enough to address concerns about women’s inequality either within the larger society or within their own organization. But, even without an articulated politics of anti-sexism, some women found in some SDS chapters an environment in which they could exercise a kind of authority available to them in few other sexually mixed institutions of the period. The question of female leadership at the chapter level of SDS needs to be further explored, but, according to Helen Garvey (who succeeded McKelvey as assistant national secretary in 1964, and who as such was SDS’s first female national officer), the chapter leaders on the SDS “key list” she dealt with were very often women.³⁵

Were the ERAP projects, with their few hundred participants, as important as Evans and Boyte suggest in providing the “free space” in which a feminist consciousness was nurtured—a consciousness only later to be injected into SDS? Or was this consciousness being generated from within SDS itself on the chapter level—and, as the Pardun letter and the McKelvey memo suggest, not just as a reaction to the sexism of men in SDS but as further expression of the group’s communal values? It may be worth recalling that the first significant protests of the 1960s involving women’s (though not yet explicitly feminist) concerns were those directed against the still prevalent campus rules serving *in loco parentis*. Parietals and other such restrictions weighed far more heavily on women than on men. It was the kind of campaign that did not show up in the pages of *New Left Notes*—the SDS National Office had nothing to do with organizing it—and, as a result, it fails to be mentioned except in passing in most histories of the movement. But, whether such protests were considered “politically correct” or not, local SDS chapters often responded more readily to the concerns of their constituents—female as well as male—than to the directives from the National Office. Such chapter-initiated protests may well prove to be one of the hidden wellsprings of the new feminism of the late 1960s.³⁶ A chapter-by-chapter history of SDS will not absolve the organization of the charge of having replicated, to greater or lesser extent, patterns of sexual inequality prevalent in the larger culture, but it might well offer new insights into the origins of the women’s movement.

³⁴ *SDS Bulletin*, 4, n.d. [1965], quoted in Rossinow, “New Left Persuasion,” 86–87.

³⁵ Garvey’s personal memories of working within the National Office were of being treated with respect by her male co-workers. Author’s interview with Helen Garvey, January 24, 1989.

³⁶ Douglass Rossinow argues for the importance of the protests against *in loco parentis*, contending that they represented the emergence of a proto-feminist sensibility at the SDS chapter level that anticipated “developments in the rest of the New Left by about two years”; Rossinow, “New Left Persuasion,” 93–95.

THE LAST THREE BOOKS UNDER REVIEW HERE, by David Farber, Ronald Fraser, and David Caute, are anniversary books, intended to commemorate the passage of two decades since that tumultuous year, 1968. They are not alone in the field: at least four other books, various conferences and reunions, and cover stories in the weekly news magazines have also marked the anniversary. *Time*, with characteristic flamboyance, described it as the year in which “the American id thrashed up into view, a spectacle of gaiety and despair.”³⁷ While Farber, Fraser, and Caute have more serious intentions in their commemorative projects than that peculiar mixture of nostalgia and condescension that has characterized the recent “Sixties boom” in the popular media, these books are not without problems of their own.

In *Chicago '68*, David Farber proposes to investigate the “crisis in the nation’s political and cultural order” revealed by the riots at the Democratic National Convention. He has burrowed into the relevant archives with the intention of providing a scholarly account of his subject. But his serious intent is badly undercut by his chosen narrative strategy. To emphasize the differing conceptions of life and politics that led to the clash in August 1968, Farber tells and then retells the story from the perspective of three of the contending camps: the Yippies, the National Mobilization Committee against the War, and the police. “In providing narratives so unadorned by the historian’s authorial voice,” Farber writes, “I gamble that the reader will address the texts I offer critically.”³⁸ To re-create the texture of the times, Farber’s “texts” employ a colloquial voice that he deems appropriate to each camp—an attempted mimicking that is imperfectly sustained and ultimately trivializing (to establish the *bona fides* of his text, Farber’s Yippies use phrases such as “stoned again”; Mayor Daley’s crowd favors homelier figures of speech such as “raise a stink”).³⁹ It is one thing to analyze the way language reflects the subjective vision of historical actors, quite another to dress up in borrowed rhetorical finery and pretend to speak through the mouths of one’s protagonists. Unintentionally, Farber’s experiment proves that the 1960s have yet to gain full acceptance as a subject for serious academic inquiry; I cannot imagine a scholarly press offering a similar monograph on, for example, the Reconstruction era, pitched in the freely rendered dialects of freedmen, carpetbaggers, and redeemers.

Chicago '68 offers its most valuable contribution in a detailed reconstruction of the planning that went on in the antiwar movement and Chicago officialdom in the months leading up to the Democratic National Convention, as both sides prepared for the showdown. There never was much doubt that “showdown” was what it would prove to be. Like *The Sixties* and “*Democracy Is in the Streets*,” *Chicago '68* is a book steeped in fatalism:

³⁷ *Time* (January 11, 1988): 17. Other recent books on 1968 include Hans Koning, *Nineteen Sixty-Eight: A Personal Report* (New York, 1987); George Katsiaficas, *The Imagination of the New Left: A Global Analysis of 1968* (Boston, 1987); Irwin Unger and Debi Unger, *Turning Point: 1968* (New York, 1988); and Charles Kaiser, *1968 in America: Music, Politics, Chaos, Counterculture, and the Shaping of a Generation* (New York, 1988).

³⁸ David Farber, *Chicago '68* (Chicago, 1988), xviii.

³⁹ Farber, *Chicago '68*, 3, 122, 259.

For Mayor Daley, a mass demonstration . . . threatened his politics, a politics in which personal loyalty outweighed loyalty to any policy . . . To the police, all the demonstrators . . . were dumb, unAmerican hippies who didn't respect the way real people lived their daily lives . . . For the antiwar movement, direct confrontation of the war makers presented a clear picture, which they knew the mass media would pass along to the public at large . . . the war makers versus the people.⁴⁰

Although Farber's personal sympathies lie more with the protesters than the police, he goes to considerable lengths to provide a balanced treatment. His main argument resembles that of some post-revisionist historians of the cold war in that he believes that neither of the two belligerent camps were as evil as their opponents thought at the time; misperceptions, failed communications, and an unwillingness to concede the humanity and good intentions of their opponents lay at the root of a tragic but overdetermined final reckoning. "The conflict at the convention was fueled by the completely different conceptions of political practice and social order the main actors brought to the planning and realization of the convention."⁴¹ The mayor and the police talked about maintaining law and order, the Yippies talked about creating a "festival of life," the organizers of the National Mobilization to End the War in Vietnam talked about peaceful, legal protests. But, with all sides unwilling and even incapable of understanding the others' concerns, violent confrontation was inevitable.

Across the Atlantic, another confrontation raged in August 1968, as the armies of the Warsaw Pact moved to bring an abrupt end to the political initiatives of the "Prague Spring" reformers in Czechoslovakia. In Britain, France, Germany, and the United States, New Leftists identified with and demonstrated their sympathy for the Czech students who were confronting Soviet tanks: "Welcome to Czechago" was one of the placards that protesters bore at the Democratic National Convention. The publication of David Caute's *Year of the Barricades* and Ronald Fraser's *1968* provides us with the means to begin to understand the political phenomenon that might be called the "fifth international," the informal network of radical student activists in Europe, America, and Asia that emerged in the course of the 1960s. In a world of porous frontiers, instantaneous communication, and mass media, it was possible for the young French revolutionaries of Nanterre to be inspired in late April 1968 by the example of the occupation of Columbia University, and for the young American revolutionaries at Columbia, in the last days of their own strike, to be heartened by the news of the May Days in Paris.

The first and better of the two studies of the international New Left is *1968*, in which interviews with 175 former New Leftists are woven into a narrative history of the student movements in the United States, Britain, Northern Ireland, France, Germany, and Italy. In putting this account together, Fraser drew on the efforts of eight other specialists in oral history. "Starting from very different positions," Fraser argues in his introduction, the European and American

⁴⁰ Farber, *Chicago '68*, xiii–xv.

⁴¹ Farber, *Chicago '68*, xvii.

student movements “progressively converged by the end of the decade.”⁴² The differences among the various New Lefts at the start of the decade were more striking than their similarities. While in the United States the New Left had no adult counterpart, on the Continent, student organizations like the Sozialistische Deutsche Studentenbund in West Germany and the various organizations of the French New Left broke away from the existing mass social democratic and communist parties. In Britain and the United States, there was a strong sense of generational identity sustained by a self-conscious youth culture; on the Continent, there was a much more pronounced sense of class grievance. The Americans were alienated from an ever-expanding university system; the Europeans wanted to open up their own cramped universities to even more students. As the decade progressed, these disparate movements increasingly came to resemble one another in their concerns, style, and strategies. Opposition to the Vietnam War gave the international New Left a common issue to organize around; the American New Left, first in the field on this issue and operating on the common enemy’s home ground, tended to set the pace and style of protest against the war. For some Europeans, particularly Catholic students in Northern Ireland, the southern black civil rights movement provided inspiration and a tactical model. The Europeans were far more sophisticated about leftist politics as it had been practiced in past decades; but it was the Americans, in their pragmatic and moralistic fashion, who seemed to be making the most significant discoveries of how leftist politics should be reordered for the future. By mid-decade, most of the influences in the international New Left flowed eastward across the Atlantic: ideas of participatory democracy, tactics of direct action, anti-bureaucratic organizational principles that originated in Berkeley and Madison would strongly influence student politics in Berlin, Paris, and London.⁴³

1968 is full of useful information and vivid anecdotes; the voices assembled here are a thoughtful if not necessarily representative sample of New Left leaders and activists. (If one were to propose a theory as to the post-1960s careers of movement activists based on the evidence of this book alone, one would have to conclude that most veterans of the American New Left became historians, while most of the French became sociologists.) Perhaps because it represents a collaborative effort, *1968* seems to have fallen victim to two usually mutually exclusive problems in oral history: too much direction or bias from interviewers shaping the responses of interviewees and, at the same time, too much special pleading by interviewees allowed to go unchallenged by the interviewers. The tendency to shape responses is evident in an interview of Jeff Jones, who recalled an SDS demonstration outside

⁴² Fraser, *1968*, 3.

⁴³ In 1965, Mike Vester, a German campus radical, wrote an article called “The Strategy of Direct Action,” which brought the recent developments in the American student movement to the attention of his comrades in the German SDS: “Its publication . . . had a tremendous effect,” he recalled, prompting German campus radicals to abandon “podium politics” for street demonstrations and occupation of buildings. Daniel Cohn-Bendit, leader of the campus occupation at Nanterre University that sparked the French May Days in 1968, recalled, “The idea of occupying came to us from America and Germany.” In the late 1960s the Vietnam Solidarity Committee was one of the main outlets for British New Left activism. According to David Widgery, a leader of the British movement, by 1968 the VSC “was using the weapons of Americanism—the theatricality, the masks”; Fraser, *1968*, 125, 189, 186.

the New York City office of the Springer newspaper chain in 1968 to protest the attempted assassination of German student leader Rudi Dutschke. The demonstration turned into a street battle with the police, involving many members of SDS's Columbia University chapter. "The militancy generated there," Jones maintained, "carried over to a week later when they occupied Columbia. It was the same people and the momentum was building."⁴⁴ While Jones's argument certainly fits in with the emphasis of the book on transatlantic cross-influences, it seems like a very idiosyncratic interpretation of the origins of the Columbia strike, and one is forced to wonder whether less internationally minded interviewers would have elicited a similar response. The tendency toward unchecked special pleading by those being interviewed is evident in the mostly benign and respectful treatment accorded the Progressive Labor party, whose former adherents claim for the group an influence within the student movement out of all proportion to its actual role, and who have correspondingly little to say about its habitual destructive sectarianism.⁴⁵ Whether the editors share their informants' interpretation in this instance is not made clear.

If there are too many such ambiguities in *1968*, there are perhaps too few in David Caute's *Year of the Barricades*. Caute documents his pronounced opinions on each constituent "party" in the New Left international, in Eastern and Western Europe, the United States, Mexico, and Asia, with considerable detail but little original research. The usefulness of the book, insofar as it deals with the events in any single country, thus stands in inverse proportion to the reader's familiarity with the standard sources. This is glaringly evident in the case of the United States, for which Caute's account is based largely on sources such as Tom Wolfe, Joan Didion, Norman Mailer, and other contemporary observers of student protest. One can almost read the footnotes and skip the text and still know what *Year of the Barricades* is going to conclude. The fact that Diana Trilling and Daniel Bell are the most frequently cited sources for the chapter on the Columbia University strike is a good indicator that Caute's sympathies are not with SDS.⁴⁶ Even these sources he has not read very carefully, as shown by the frequent minor errors that creep in.⁴⁷

⁴⁴ Fraser, *1968*, 195.

⁴⁵ Fraser, *1968*, 160, 292, 294, 310.

⁴⁶ Bell is cited six times, Trilling five in Caute's footnotes on the Columbia strike. No other source is cited more than once. Caute's account of the initial occupation of Low Library is typical of his tone throughout the chapter: "It was a time for heroes, but the group occupying the president's office dropped out of the window one by one (including Mark Rudd) until only 27 were left to face the music . . . In a bizarre turn, the police shrugged and abandoned Low Library, taking the Rembrandt with them, while SDS began climbing back through President Kirk's window"; David Caute, *The Year of the Barricades: A Journey through 1968* (New York, 1988), 167, 480–81. However amusing, Caute's account might have benefited by conversations with some of the actual participants who remember that first morning in Low Library differently. Their recollections are of a confused scene with a lot of students milling around, some leaving the building because they were convinced the occupation was a mistake, others leaving to organize more support on campus; not the panicked flight that Caute, citing Bell as his sole source, suggests. Author's interview with Mark Erlich and Barbara Gottlieb, January 15, 1989.

⁴⁷ For example, Caute describes Mississippi governor Ross Barnett as a "southern sheriff," mistitles and attributes Woody Guthrie's "This Land Is Your Land" to Pete Seeger, transforms Columbia University's Student Afro-American Society (SAS) into the "Afro-American Society (A-AS)," and renames Barnard sophomore Linda LeClair (who made front-page headlines in the *New York Times*

In contrast to his often derisory treatment of the American New Left, Caute lionizes the French New Left and regrets that the French Communist party was unwilling to proceed to the revolutionary seizure of state power in the midst of what was “the greatest popular insurrection ever experienced by capitalist democracy in time of peace”: “Let us imagine that the strike wave had been allowed to continue—had been actively encouraged and coordinated by the unions—and that provincial disorders had gathered momentum . . . Could the government have survived? Could de Gaulle have re-enacted the massacre of 1871? Could he have turned Paris into ‘Budapest 1956’?”⁴⁸

Caute seems most sympathetic to the *enragés* of the French New Left because their attempted revolution most clearly reflects the influence of the classical model. Where revolution was not on the agenda, or part of the national tradition, as in Britain and the United States, Caute sees mainly the self-delusion and excess of “radical chic” at work: these New Leftists—and particularly the Americans—sprang from the very consumer culture they pretended to subvert. “[I]nstant kinetics were up for grabs both for street fighters and the smart, moneyed people who patronized galleries and bought the new furniture.”⁴⁹ The end result, again more or less preordained (except in France, where the proper exertion of revolutionary will might have swept all obstacles before it), was confusion and ideological capitulation. Caute concludes: “This constant cultural neutering of dissent, the eclecticism which treats all ideas as short-lived merchandise, has reinforced the triumph of the profit motive and the idolization of market forces in the era of Reagan and Thatcher.”⁵⁰ Even *Year of the Barricades*, which of all these books is the most scathing in its treatment of the American New Left, finds fault from the Left rather than the Right; the problem was that the New Left was insufficiently committed to its own revolutionary project and thus found itself outflanked and assimilated.

CONTEMPORARY POLITICAL CONCERNS inevitably color the interpretations being offered today of the 1960s: a fact that is no less true for Gitlin, Miller, Farber, Fraser, and Caute than it is for George Bush. Like Elinor Langer in her undergraduate days, contemporary historians of the New Left write from a

in the spring of 1968 when it was revealed that she was living off campus with her boyfriend) as “Linda St. Clair.” Caute, *Year of the Barricades*, 51, 147, 165, 166.

⁴⁸ One need not hold the French Communists in particularly high regard to answer Caute’s hypothetical question, “Quite possibly yes.” A question that Caute fails to pose is whether the United States and the other NATO allies would have stood by idly while a communist revolution took place in France. Caute, *Year of the Barricades*, 212, 250, 254–55.

⁴⁹ Caute, *Year of the Barricades*, 311.

⁵⁰ Caute, *Year of the Barricades*, 462. Not only Caute’s analysis here but also his language is more than a little derivative of Christopher Lasch’s writings. Lasch describes one-time Yippie leader Jerry Rubin moving to San Francisco in the mid-1970s, “where he shops voraciously on an apparently inexhaustible income in the spiritual supermarkets of the West Coast.” Caute’s version describes Rubin’s “voracious shopping expedition in the spiritual supermarket.” Christopher Lasch, *The Culture of Narcissism: American Life in an Age of Diminishing Expectations* (New York, 1979), 14; Caute, *Year of the Barricades*, 475.

perspective that suggests they too have difficulty in understanding how anyone could be so stupid as not to wish revolutions—or, at least, “movements for social change”—well. “Say what we will about the Sixties’ failures, limits, disasters,” Gitlin argues in the conclusion to *The Sixties*, “America’s political and cultural space would probably not have opened up as much as it did without the movement’s divine delirium.”⁵¹

In what Gitlin calls the “long sputtering” of the movement’s decline in the early 1970s, many New Leftists retreated to private preoccupations—“J’accuse to Jacuzzi” in the memorable phrase from *The Sixties*—but there was more to the legacy of the decade’s insurgencies than chastened ex-radicals returning to the mainstream.⁵² Gitlin, like many of the recent historians of the 1960s, seems to suggest that some huge, necessary, and painful shift in American culture had been taking form for some time before the 1960s; through a combination of the default of other actors and historical accident, the New Left became the unconscious agent of this shift in the “Zeitgeist,” which brought with it new attitudes toward race, gender, work, family, authority, and the state. The New Left, it might be said, failed politically in the short run but triumphed culturally in many ways in the long run.⁵³ And, as far as some of the authors under review here are concerned, it may prove entirely premature to sweep the New Left generation into the political dustbin. “The Sixties returns are not in,” Gitlin suggests on the last page of his book, while Fraser concludes that the events of 1968 “showed that, from small beginnings and scattered hopes, social movements can arise, and arise very rapidly to challenge the established order. It is this that a generation barely reaching forty today has experienced and remembers.”⁵⁴

Given the pace of historical revisionism in other fields, it would be foolish to expect that the current serene consensus in New Left history will remain in place forever—or even very far into the 1990s. If and when more conservative or otherwise critically inclined historians turn their attention to the New Left, they might take as their first project the restoration of a sense of human agency, choice, and culpability to this history, all of which has tended to disappear in the predestinarian interpretations offered by Gitlin and others. What should be retained from the current wave of books on the 1960s is the sense that the American New Left’s sins—however damning they may appear in these and future treatments—were of the homegrown variety. In a brief essay published in 1984, writing from the perspective of both historian and participant, James Gilbert made a shrewd and essential point about the New Left’s place in American history:

The impulse to expect and demand perfection in politics and society, to live by absolutes alone, is an old tradition that wends its way in American history, through Puritanism,

⁵¹ Gitlin, *The Sixties*, 435.

⁵² Gitlin, *The Sixties*, 433.

⁵³ These themes are developed at greater length in Maurice Isserman and Michael Kazin, “The Failure and Success of the New Radicalism,” in Steve Fraser and Gary Gerstle, eds., *The Rise and Fall of the New Deal Order, 1930–1980* (Princeton, N.J., 1989), 212–42.

⁵⁴ Gitlin, *The Sixties*, 438; Fraser, 1968, 356.

Perfectionism, Teetotalism, Prohibition, and a thousand other reforms and schemes. What we did not recognize at the time was the hidden Americanism of our demands: the spiritual slogans about equality, the abhorrence of privilege and elitism; the burning intensity of our rejection of the old—meaning compromised—left. In fact, nothing could have been more traditional or in character.⁵⁵

To understand the 1960s, one needs to grasp the paradoxical character of the decade. On the one hand, the emergence of the New Left, and the broader spirit of social inquiry, activism, and protest of which it was a part, was the product of an unprecedented and unrepeatable conjunction of factors, including the sustained economic boom and the demographic baby boom of the postwar years, the expansion of higher education and the courtship of young consumers by Hollywood and the record industry, the emergence of the southern civil rights movement and its embrace of direct-action tactics, the discrediting and collapse of the Old Left, the election of John F. Kennedy, and the waning of direct Soviet-American international confrontation. On the other hand, as Gilbert suggests, the New Left's spirit of moral perfectionism and millenarian enthusiasm was hardly unique in the history of American reform. The story of the 1960s in the United States, even as it has been told by sympathizers with the New Left, reveals more than a little folly and delusion—but, as *Time* magazine now concedes, it was a distinctly "American" id that came thrashing up into view in those years.

⁵⁵ James Gilbert, "New Left: Old America," in Sohnya Sayres, *et al.*, eds., *The 60s without Apology* (Minneapolis, Minn., 1984), 246.

Review Article
**Objectivity and Historicism:
A Century of American Historical Writing**

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Peter Novick, **That Noble Dream: The "Objectivity Question" and the American Historical Profession** (Cambridge: Cambridge University Press, 1988).

REASON ITSELF IS HISTORICAL. That insight, which theorists from a wide range of disciplines have come to share in the last two decades, should be welcomed by professional historians as the good news of postmodernism. Instead, many historians have greeted with distrust or disdain the conception of rationality as dialogical and progressive. When radical historicism threatens to topple reason itself from its transhistorical perch, many working historians rush to the rescue. They understand that the commonsensical notion of objectivity inspiring their work is under attack. So, as scholars from various disciplines hasten to repudiate earlier efforts to discover Timeless Truths and Universal Laws, and work diligently to adjust their own vision to include the ceaseless flow of ideas through time, many professional historians retreat to a fortress of unexamined assumptions concerning the nature of the knowledge they can provide.¹ As Peter Novick demonstrates, these issues have been raised before. The problems of objectivity and interpretation have been present as long as history has been written; the solutions now being offered can be traced to the first crisis of historicism in the late nineteenth century.

When the American Historical Association celebrated its fiftieth anniversary in 1934, Theodore Clarke Smith surveyed the work of the profession. His fellow historians, he proclaimed, had demonstrated since 1884 their steadfast allegiance to "the ideal of the effort for objective truth." A year later, Charles A. Beard reaffirmed the very different version of history he had outlined in his 1933 presidential address to the AHA and responded to Smith that the "noble

¹ For overviews of these developments, see Richard J. Bernstein, *Beyond Objectivism and Relativism: Science, Hermeneutics, and Praxis* (Philadelphia, Pa., 1983); John Rajchman and Cornel West, eds., *Post-Analytic Philosophy* (New York, 1985); Kenneth Baynes, James Bohman, and Thomas McCarthy, eds., *After Philosophy: End or Transformation?* (Cambridge, 1987); and Richard Harvey Brown, "Positivism, Relativism, and Narrative in the Logic of the Historical Sciences," *AHR*, 92 (October 1987): 908–20.

dream” of scientific objectivity was but an illusion. Writing history, Beard insisted, is “an act of faith.” Despite all efforts to know the past, the historian “remains human, a creature of time, place, circumstance, interests, predilections, culture,” characteristics that shape historical work and are inevitably reflected in it.²

Fifty years after that exchange, Arthur S. Link benignly assessed the progress of the AHA during its first century in his presidential address. Link congratulated the profession for “the range and diversity” of its work and applauded its ever-broadening membership and horizons of scholarly concern. A year later, AHA President William H. McNeill struck a rather different chord. He dismissed “historiography that aspires to get closer and closer to the documents—all the documents and nothing but the documents,” because such work “is merely moving closer and closer to incoherence, chaos, and meaninglessness.” He called for historians to confront their limitations and acknowledge that they produce not “eternal and universal Truth” but “mythistory.” While McNeill expressed his belief that such “ever-evolving mythistories will indeed become truer and more adequate to public life,” that hope, he conceded, echoing Beard, was “an act of faith.”³

Responses to Peter Novick’s important study of the objectivity question and the American historical profession will vary widely. Historians drawn to Smith’s and Link’s positions on historical truth and the social history of the AHA will find their ideas challenged; those persuaded by Beard and McNeill will find much evidence confirming their views. But partisans and critics of the idea of historical objectivity, as well as defenders and prosecutors of the profession itself, should read Novick’s book. It contains a wealth of information about the shifting fortunes of historical truth and the changing role of historians in American culture.

Using the published and unpublished writings of hundreds of historians, Novick provides a detailed chronicle of the American historical profession as it has passed through four phases. This framework, whose artificiality Novick readily admits, provides a heuristic device that brings some degree of order to the diverse and unwieldy materials he discusses. First, from 1880 to 1917, objectivity was enthroned as “the central norm of the profession.” The struggle evident in some professionalizing social sciences between advocacy and objectivity did not develop among historians, Novick claims, because historians in this early phase took for granted their commitment to disinterested inquiry. The first generation of professional historians rewrote much of the nation’s history to suit its white, Anglo-Saxon, Protestant assumptions. Slavery became an understandable accommodation with difficult circumstances, the American Revolution a result of the colonists’ impatience and ingratitude, and Reconstruction a tragedy

² Theodore Clarke Smith, “The Writing of American History in America, From 1884 to 1934,” *AHR*, 40 (April 1935): 445; Charles A. Beard, “That Noble Dream,” *AHR*, 41 (October 1935): 74, 83; Charles A. Beard, “Written History as an Act of Faith,” *AHR*, 39 (January 1934): 219–31.

³ Arthur S. Link, “The American Historical Association, 1884–1984: Retrospect and Prospect,” *AHR*, 90 (February 1985): 8; William H. McNeill, “Mythistory, or Truth, Myth, History, and Historians,” *AHR*, 91 (February 1986): 8–9.

of excessive reformist zeal. As Novick points out, all these judgments—racial, cultural, and political—came wrapped in a cloak of “science” that shielded them from partisan criticism. A consensus of “conservative evolutionism” effectively muffled the voices of “a most genteel insurgency” daring to raise questions about that presumption of objectivity.⁴

In the second phase of Novick’s account, the “changed cultural, social, and political climate” of the interwar years permitted relativism to flourish, however briefly. Critics such as Beard and Carl Becker challenged the orthodoxy of objectivism, and modernist ideas at last filtered slowly into the historical community. New intellectual currents, however, could not prevent the profession from stagnating during these years. Books were often published in the 1930s only if authors (or their friends) could afford to subsidize them. Jews, Catholics, atheists, and agnostics were formally or informally barred from universities. Historians who were employed saw their salaries and status sink. Private control of many faculty appointments and organized assaults on “subversive doctrines” enforced political conformity. Professional historians lost control of the social studies curriculum in secondary schools to the new educational establishment. They lost their lay audience to gifted amateurs who wrote popular histories. The bleakness of most historians’ situations was matched only by the barrenness of the work they produced. Perhaps mercifully, “incestuous back-scratching and mutual admiration,” in David Potter’s words, “chloroformed the review sections of American historical journals” during these years. Novick limits his discussion of historiographical disputes to the controversies swirling around German war guilt and the coming of the American Civil War, both of which seemed increasingly to turn on questions of personal commitment rather than scientific verification. In a glum summary of the interwar period, Novick writes, “declining confidence in professionalism helped call its associated doctrine [of objectivity] into question.”⁵

The coming of World War II reversed the profession’s decline and bolstered historians’ faith in their ability to disclose truth. Relativism in all fields came under attack, as scholars denounced cultural anthropology, legal realism, pragmatism, and the sociology of knowledge for making possible the nihilism that bred totalitarianism. Historians assailed Becker and especially Beard, because of his isolationism even more than his relativism. The doubts of the interwar years vanished in the resolute affirmation of Americanism that sustained the nation through the ordeal of the war and into its triumphant and then traumatic aftermath. As Novick indicates, both conservatives invoking absolute truth and liberals trumpeting their nonpartisanship challenged the wide range of ideas that had eroded faith in the ideal of objectivity. During the late 1940s and 1950s, historians confidently constructed a “new, somewhat chastened, objectivist synthesis, trivializing the relativist critique by partially incorporating

⁴ Peter Novick, *That Noble Dream: The “Objectivity Question” and the American Historical Profession* (Cambridge, 1988), 16, 86–87.

⁵ Novick, *That Noble Dream*, 16, 200 n. 57, 205. The quotation from David Potter is taken from Potter’s review of David Hackett Fischer, *Historians’ Fallacies*, *Journal of Southern History*, 37 (1971): 87; it appears on 202–03 of *That Noble Dream*.

it.” During these years, the number of professional historians rose, anti-Semitism and anti-Catholicism fell, and the quality and quantity of published work improved. With many of its prominent members having served proudly in the Office of Strategic Services during the war, Novick points out, the AHA quickly and easily adopted the new realism of the cold war years. American diplomatic history became the story of America’s rise to power. General history courses became the story of Atlantic civilization’s rise to dominate the uncivilized non-Western world. Throughout the scholarly community, efforts proceeded simultaneously to mobilize the West “for world-wide ideological struggle, while parading disinterested objectivity as one of the West’s distinctive values and institutions.”⁶

The 1960s saw the weakening of this conviction. “The political culture lurched sharply left, then right; consensus was replaced first by polarization, then by fragmentation; affirmation, by negativity, confusion, apathy, and uncertainty.” This chaos has persisted into the present, and Novick makes no attempt to impose order on it. He recounts the emergence of New Left, black, ethnic, and feminist scholarship, the rise of the field of public history, and the proliferation of narrow sub-specialties with their own conferences, journals, and jargons. He describes the shrinking job market, the dwindling purchasing power of professors’ salaries, the eroding authority of the professoriat vis à vis university administrators, and the increasing marginalization of history in the schools. He cynically dismisses the papers presented at the annual meetings of the AHA as pointless rituals, mere distractions from the real purpose of such conferences: job hunting. Finally, he argues that specialization has advanced to the point that calls by the editors of the *American Historical Review* for submissions of general interest are meaningless, since “there no longer [are] any topics of general interest.”⁷ The breadth of the appeal of Novick’s book itself, however, decisively refutes that claim.

Novick’s detailed chronicle of the American historical profession constitutes an important contribution that will be of interest to all historians, but on several different levels. As C. Vann Woodward has noted, older historians will be intrigued (or distressed) to see their teachers, then their peers, then their younger colleagues, and finally their students emerge as principal players in the drama Novick recounts.⁸ Younger historians may find the book a reprise of their comprehensive examinations, played on fast forward. The book is a gold mine of inside stories, revealing the private thoughts of many prominent and not-so-prominent historians, and for that reason it will have—as it deserves—a wide audience. It may also inhibit historians from committing their private thoughts to paper in the future. The publication of Novick’s book, together with the easy, confidential access to others that long-distance telephone calls provide, may make it impossible to write the sequel to this volume fifty years from now.

⁶ Novick, *That Noble Dream*, 16, 314.

⁷ Novick, *That Noble Dream*, 415, 580.

⁸ C. Vann Woodward, “Truth and Consequences,” *New Republic* (February 20, 1989): 40–43.

IF THE PRINCIPAL STRENGTH of Novick's study is its social history of the American historical profession, its principal weakness is the intellectual history of the objectivity question that it does not provide. Those familiar with the existing secondary literature on the American historical profession may wonder why Novick ignores studies such as Gene Wise's *American Historical Explanations* and David Hackett Fischer's *Historians' Fallacies*, which cover much of the same ground. Moreover, although Novick does refer to studies such as Thomas L. Haskell's *Emergence of Professional Social Science* and John Higham's *History*, he does not advance our understanding of the substantive issues concerning the question of objectivity itself. To be fair, Novick explicitly renounces the attempt to provide systematic analysis of that sort. He notes in his introduction that the idea of historical objectivity rests on philosophical ideas he considers "dubious" and that important aspects of the idea seem to him "psychologically and sociologically naive." He explains "the reason why I cannot take a position for or against objectivity is my historicism, which here means simply that my way of thinking about anything in the past is primarily shaped by my understanding of its role within a particular historical context, and in the stream of history." He announces that his "own deepest methodological commitment is to the 'overdetermination' of all activity, including thought," but beyond that he refuses to go. To elaborate further would involve defending his position "with arguments," which he chooses not to do "since this isn't that sort of book."⁹ This is, however, that sort of essay, and readers should be forewarned that I do intend to defend with arguments, albeit in shorthand form, a version of historicism unlike the one that Novick espouses, a pragmatic hermeneutics that provides an alternative to the gloom that descends over the concluding chapter of *That Noble Dream*. Novick's panorama of the profession is impressive, and it provides exactly the comprehensive overview of the shaping of the professoriat that he set out to provide. It would be unfair to fault him for failing to write a book altogether different from the one he chose to write. He has produced a solid, useful social history of an important topic, and this essay should not be understood as a denial of that considerable achievement. I want only to explore another dimension of the objectivity question, particularly because I intend to offer a rather different perspective from Novick's on the problems and prospects facing contemporary historians who confront these issues.

The historicism Novick espouses has a history itself, a history roughly parallel to the history he offers in *That Noble Dream*.¹⁰ Some thinkers who have ventured

⁹ Gene Wise, *American Historical Explanations: A Strategy for Grounded Inquiry*, 2d edn. (Minneapolis, Minn., 1980); David Hackett Fischer, *Historians' Fallacies: Toward a Logic of Historical Thought* (New York, 1970); Thomas L. Haskell, *The Emergence of Professional Social Science: The American Social Science Association and the Nineteenth-Century Crisis of Authority* (Urbana, Ill., 1977); John Higham, *History: Professional Scholarship in America* (New York, 1965). Novick, *That Noble Dream*, 6, 7, 9.

¹⁰ On historicism, see Leonard Krieger, *Time's Reasons: Philosophies of History Old and New* (Chicago, 1989), 107–35; and Michael Ermarth, *Wilhelm Dilthey: The Critique of Historical Reason* (Chicago, 1978). Due largely to the influence of Martin Heidegger and Hans-Georg Gadamer, contemporary writers sometimes assume there is a contradiction between historicism and hermeneutics. A recent example of this misconception is David Harlan, "Intellectual History and the Return of Literature," *AHR*, 94 (June 1989): 581–609. In his response to Harlan, "The Return of the Prodigal: The Persistence of Historical Knowing," *AHR*, 94 (June 1989): 610–21, David A. Hollinger emphasizes

critiques of objectivity have developed ideas considerably more elaborate than the rough-and-ready commitment to the overdetermination of thought Novick offers. Near the end of his introduction, he notes, almost apologetically, that “inevitably, I spend a good deal of time talking about what historians do worst, or at least badly: reflecting on epistemology. This loads the dice against historians: something like a sportswriter reporting on their performance in the annual history department softball game.”¹¹ By providing the account he does, however, Novick also loads the dice against those historians who have played more than a little softball in their day. All readers of the book will derive from it rich anecdotes about the follies, misconceptions, and pettiness of notable historians who just could not play epistemology very well. Unfortunately, by refusing to examine in detail the more sophisticated versions of historicism that provide attractive alternatives to objectivism, Novick ironically may perpetuate the very hyper-realism he distrusts. He devotes considerably more attention to the archival “finds” that objectivists prize than to the interpretation of difficult ideas that historicists have tried to develop. Novick’s study demonstrates, in short, that a long and multidimensional portrait can appear to be an objective chronicle of “what actually happened” to the American historical profession, even though it seems clear from his introduction that Novick sought to do something quite different. Novick’s periodization, though useful for his social history of historians, contributes to this problem by blurring the edges of the ideas advanced by thinkers who were out of phase with the historical profession, thinkers whose insights undermined the idea of objectivity even though they did not succeed in converting the majority of working historians to their point of view.

Novick’s account of the relation between the idea of objectivity and a vague alternative of relativism seems to me inadequate. The philosopher Richard J. Bernstein has distinguished between two approaches to knowledge, what he terms the “grand and seductive Either/Or,” on the one hand, and efforts to move “beyond objectivism and relativism,” on the other. Bernstein’s brilliant analysis concerns recent philosophical work, particularly the ideas of Hans-Georg Gadamer, Jürgen Habermas, Richard Rorty, and Hannah Arendt. He realizes, though, that his argument can be extended back to the early twentieth-century ideas of William James, John Dewey, Ludwig Wittgenstein, and Wilhelm Dilthey,

the symbiotic relationship between contextualist and (Dilthey if not Gadamerian) hermeneutic approaches to historical study; see esp. 613–14. For a more detailed account of the varieties of hermeneutics, and an explanation of the reasons why historians might still find Dilthey’s version useful, see the splendid essay by Michael Ermarth, “The Transformation of Hermeneutics: 19th-Century Ancients and 20th-Century Moderns,” *The Monist*, 61 (April 1981): 176–94. Dilthey took not only texts but all of experience as the appropriate field of hermeneutical inquiry. His method was not limited to the boundaries of linguistic expression that provide the horizon for so much contemporary scholarship. As Kurt Mueller-Vollmer has written in the introduction to his comprehensive overview of this tradition since the eighteenth century, *The Hermeneutics Reader* (Oxford, 1986), 27, Dilthey believed that “the human sciences had as their object the interpretation of all phenomena, nonverbal as well as verbal.” For reasons I hope to make clear, that approach has more in common with Deweyan pragmatism than with Derridean deconstruction; it may also prove more useful for historians.

¹¹ Novick, *That Noble Dream*, 15.

all of whom likewise sought to move beyond the dichotomies that constrain Novick's analysis. Novick begins his book by briefly listing the cluster of ideas that compose the concept of historical objectivity: "a commitment to the reality of the past, and to truth as correspondence to that reality; a sharp separation between knower and known, between fact and value, and above all, between history and fiction. Historical facts are seen as prior to and independent of interpretation . . . Truth is one, not perspectival. Whatever patterns exist in history are 'found,' not 'made.'" This conception of binary oppositions between subject and object or fact and interpretation can be traced back to what Bernstein calls the "Cartesian anxiety" at the root of modern Western thought: "Either there is some support for our being, a fixed foundation for our knowledge, or we cannot escape the forces of darkness that envelop us with madness, with intellectual and moral chaos." These pervasive dualisms haunted seventeenth, eighteenth, and nineteenth-century thinkers who believed that, without an epistemological Archimedean point, all knowledge would be lost to the anarchy of competing opinions. As Richard Rorty has argued persuasively in *Philosophy and the Mirror of Nature*, such dichotomies threw Western philosophy off course for several centuries.¹²

In the closing decades of the nineteenth century, however, a number of European and American philosophers tried to escape those dichotomies by reconceiving the relations between mind and body, subject and object, fact and value, and knowledge and action. Having already written a long study of these ideas, I will offer only a short summary here. These thinkers advanced a radical theory of knowledge that broke down inherited dualisms and replaced them with a conception of immediate experience as the intersection of subject and object, a pragmatic theory of truth that substituted continuing social experimentation for certainty, and a historical sensibility that conceived of all knowledge—in the natural as well as the social sciences—as intrinsically meaningful and rooted in cultural processes that can be known only through interpretation. Their perspective, in short, was phenomenological, pragmatic, and hermeneutic, and it contrasted in every dimension with the binary opposition between objectivism and relativism that is fundamental to Novick's study.¹³

If Bernstein and Rorty are right about the contemporary scene, the voyage "beyond objectivism and relativism" will soon be standard for historians as well as philosophers, political and social theorists, and perhaps even a few stray literary critics. Dissatisfaction with objectivity need not culminate in relativism or in skepticism. There is an alternative, which John Dewey described in a lecture on William James that he delivered in 1920:

James has consistently opposed absolute dogmatism in philosophy, and at the same time he has repudiated utter skepticism. Even though he recognized that no truth can be

¹² Bernstein, *Beyond Objectivism and Relativism*, 16–20; Novick, *That Noble Dream*, 1–2; Richard Rorty, *Philosophy and the Mirror of Nature* (Princeton, N.J., 1979); see also Richard Rorty, *Consequences of Pragmatism* (Minneapolis, Minn., 1982).

¹³ James T. Kloppenberg, *Uncertain Victory: Social Democracy and Progressivism in European and American Thought, 1870–1920* (New York, 1986), 15–114.

discovered unless there is an inclination to doubt, he eschews absolute skepticism because it is not constructive. Skepticism, according to James, can be justified only when it advances an alternative hypothesis; and if the skeptic's hypothesis is verified, we must accept it in lieu of the earlier one which gave rise to the skeptic's doubts. What is most distasteful to James is a skepticism which brings with it nothing that can contribute constructively to investigation. He advises us to doubt, but he warns us against an attitude of complete skepticism. He asks us to look for new truth in the results of our past experiments at the same time that we continue to experiment and to seek for a growing area of practical belief.¹⁴

As James understood and Dewey reaffirmed, there is in pragmatic theory a fruitful alternative to relativism. Hypotheses—such as historical interpretations—can be checked against all the available evidence and subjected to the most rigorous critical tests the community of historians can devise. If they are verified provisionally, they stand. If they are disproved, new interpretations must be advanced and subjected to similar testing. The process is imperfect but not random; the results are always tentative but not worthless. It is this strand of pragmatic hermeneutics, which has been present in the best work of American historians since the first decade of the twentieth century, that Novick tends to overlook. When all historians are categorized as either objectivists or relativists, this intermediate position may either disappear from view or seem a tepid compromise. I want to examine briefly several of the historians Novick discusses in *That Noble Dream*, suggesting in each case why his version of the “Either/Or” leads him to misconstrue their conception of historical truth.

NOVICK CHARACTERIZES JAMES HARVEY ROBINSON as a “moderate skeptic” on the objectivity question, but he contends that a “significant qualification” makes it appropriate to distinguish Robinson's writing prior to 1917 from relativism. History, Robinson wrote, “should not be regarded as a stationary subject which can only progress by refining its methods and accumulating, criticizing, and assimilating new material.” The relativity of historical knowledge “is conditioned by our constant increase in knowledge . . . To what may be called the innate relativity of things . . . we have added a dynamic relativity which is the result of rapidly advancing scientific knowledge, which necessarily renders all our conclusions provisional.” This passage seems clearly to reflect the influence of James, whose powerful and lasting effect on Robinson Novick notes and Robinson himself acknowledged, and of Dewey, with whom Robinson frequently met after Dewey moved to Columbia University in 1910, as Novick also mentions. Because Robinson expressed “confidence in progress,” however, Novick judges his and other such optimism “a powerful limitation on the critique of historical objectivity.”¹⁵ Yet, as Dewey's 1920 assessment of James correctly indicates, pragmatists insisted on the “constructive” dimension of inquiry. James

¹⁴ John Dewey, “William James,” a lecture first delivered in Peking in 1920, in Dewey's *Middle Works, 1899–1924*, vol. 12, 1920, ed. Jo Ann Boydston (Carbondale, Ill., 1982), 220.

¹⁵ James Harvey Robinson, *The New History* (New York, 1912), 15, 130; Novick, *That Noble Dream*, 105.

and Dewey both denied that pragmatism must culminate in skepticism; they endorsed it precisely because it seemed to them likely to disclose provisional but useful knowledge that could make possible further progress. From the earliest stages of “the new history,” then, when Robinson, Beard, and Becker were beginning to challenge historical objectivity by infusing their historical writing with Jamesian ideas of truth, the possibility of a pragmatic hermeneutics was under consideration by American historians. This modification does not quite fit Novick’s conception of a pre-war objectivist consensus, but it seems to me nevertheless a necessary refinement of his scheme.

Novick’s account of the challenges to objectivity in the 1930s is among the strongest sections of *That Noble Dream*. He discusses briefly and deftly the various intellectual currents that bolstered Beard’s and Becker’s confidence in their anti-objectivist critiques. From the pragmatism of James and Dewey and the new science of Albert Einstein, Niels Bohr, and Werner Heisenberg to the cultural anthropology of Franz Boas, Ruth Benedict, and Margaret Mead, the anthropological linguistics of Edward Sapir and Benjamin Whorf, and the legal realism of Karl N. Llewellyn, Jerome Frank, and Thurman W. Arnold, Novick outlines the ideas that unsettled interwar claims to objective truth. He traces the influence of Wilhelm Dilthey, Benedetto Croce, and Karl Mannheim on American thinking. He examines Beard’s and Becker’s influential interwar writings thoroughly and incisively. Moreover, when discussing the criticism they received from historians such as Theodore Clarke Smith and philosophers such as Maurice Mandelbaum, Arthur O. Lovejoy, and Felix S. Cohen, Novick emphasizes a distinction similar to that Dewey drew between skepticism and James’s pragmatism. Becker and Beard “did not deny the existence or knowability of truth,” Novick concedes. “Rather than saying that there was no criterion for what is true, they stressed the variety of criteria employed by different societies, epochs, and methodological assumptions.” They denied only “the existence of a timeless and universally valid metacriterion for deciding between them. The distinction between skepticism and relativism, real and important to Becker, Beard, and those in their camp, seemed unreal and inconsequential to absolutists for whom ‘truth,’ to be worthy of the name, had to be singular and immutable.”¹⁶

Important as that distinction between relativism and absolutism might seem to Novick, it may be somewhat misleading. Neither Beard nor Becker considered himself a “relativist” in the sense in which Novick seems to use the term. Instead, they were historicists—and pragmatists—through and through. Beard branded relativism a species of “absolutism” and countered it with “the absolute totality of all historical occurrences past, present, and becoming to the end of all things.” Given the unfinished quality of experience, given what James called its open-endedness, relativism—like all other doctrines—must withstand the unending test of verification in practice. In Beard’s words, even though history can never be a science because it will never have predictive power, historians will continue to rely on the scientific method to frame and test their hypotheses, because “the

¹⁶ Novick, *That Noble Dream*, 263.

inquiring spirit of science, using the scientific method, is the chief safeguard against the tyranny of authority." For Becker, getting the facts right will always remain a historian's duty, while the idea of a science of history will always remain an illusion. "Regarded historically, as a process of becoming," Becker wrote, echoing James, "man and his world can obviously be understood only tentatively, since it is by definition something still in the making, something as yet unfinished." History as Beard and Becker understood it was not so much relativism as it was a form of hermeneutics and pragmatic truth-testing, in which knowledge derives from weaving together fact and interpretation to create stories—myths, Becker called them—whose accuracy must therefore always be considered provisional.¹⁷

Such ideas attracted considerable support during the 1930s, but the coming of the Good War changed everything. Becker and especially Beard were buried under a torrent of abuse from prominent historians such as Allan Nevins, J. H. Hexter, Thomas A. Bailey, and Samuel Eliot Morison. But, as Novick argues, it was perhaps less their historicism than Beard's isolationism and his criticism of the sainted Franklin D. Roosevelt that accounted for the postwar outpouring of ill will. The consequences of relativism were pernicious, Beard's critics charged. If the critique of objectivism led to follies such as pacifism in the face of Fascism and Nazism, then objectivism must be preferable to the alternatives. Equally sloppy logic undergirded most self-righteous affirmations of Americanism as well as objectivism, and in the wake of several rather less good wars it requires an act of historical imagination to recapture the cultural self-confidence that fueled historical writing in the 1950s.¹⁸

It would be a mistake, however, to assume that the objectivist doctrine simply returned to its Rankean shape after having been stretched and rumped by critics from a variety of disciplines. Even Morison, the admiral-historian of impeccable credentials in both hot and cold wars, took the occasion of his AHA presidential address in 1950 not only to pay his disrespects to Beard, as Novick notes, but also to distance himself from the notion of scientific history. "It goes without saying," Morison made a point of saying, "that complete, 'scientific' objectivity is unattainable by the historian . . . No historian of my generation has ever pretended otherwise." While such concessions elicit Novick's judgment that postwar historians trivialized the relativist critique by partially incorporating it, many of them did more than that. Morison, for example, cited approvingly not only Leopold von Ranke but Croce, and he acknowledged that it is the historian who "decides what is significant, and what is not . . . because he is writing of the

¹⁷ Beard, "Written: History as an Act of Faith," 225, 227; Carl Becker, "Everyman His Own Historian," *AHR*, 37 (January 1932): 236, 231. Compare the quite different interpretation of Beard's and Becker's positions in Novick, *That Noble Dream*, 278. A wide-ranging study of Beard is Ellen Nore, *Charles A. Beard: An Intellectual Biography* (Carbondale, Ill., 1983).

¹⁸ Novick's perceptive account of this period relies heavily on Edward A. Purcell, Jr., *The Crisis of Democratic Theory: Scientific Naturalism and the Problem of Value* (Lexington, Ky., 1973); see also Richard H. Pells, *The Liberal Mind in a Conservative Age* (New York, 1985); and John Patrick Diggins, *The Proud Decades: America in War and in Peace, 1941–1960* (New York, 1988).

past but not *for* the past; he is writing for the public of today and tomorrow.”¹⁹ Morison distanced himself from “relativism,” to be sure, but so did Beard, despite the charges of his critics. Historians had as much reason to be proud, and to be chastened, as did other Americans in the 1940s and 1950s, and the stridency of their nationalism is understandable in that context. Incorporating critiques of objectivism, however, does not necessarily involve trivializing them. Sophisticated versions of historicism do not require historians to renounce their own values but only to confront them openly, as Morison and other cold warriors occasionally did. Beard and Morison were equally forthright partisans politically, although they often took different sides. As historians, they occupied the same scarred and contested terrain somewhere between—or perhaps even beyond—objectivism and relativism. When Morison criticized some of the more egregious errors in Beard’s historical writing, he was just as clearly on target as Beard was in criticizing the excessive objectivism of ostensibly scientific historians.

AMERICAN HISTORIANS IN THE POSTWAR YEARS shared the national consensus on the desirability of democracy and the identification of American culture as it was with their democratic ideal. Novick’s evidence indicates that their consensus did not extend to the objectivity question. He recounts the diversity of opinion that greeted drafts of Bulletin 54 of the Social Science Research Council, *Theory and Practice in Historical Study: A Report of the Committee on Historiography*, which were circulated in the late 1940s. This compilation of essays and propositions, inspired by Beard and directed by Merle Curti, staked out a position somewhere between objectivism and relativism. On the one hand, writing history requires “a selection of so-called facts made by some man or woman . . . under the influence of some scheme of reference, interest, or emphasis.” On the other hand, the historian “must confine himself to the making of verifiable statements about history-as-actuality.” Although both statements seem to me fully consistent with the historicist conception of pragmatic hermeneutics, to Novick the imperative for historians to confine themselves to the verifiable “seemed to contradict and undercut” the “clearly relativist . . . principal thrust” of the document. If the document itself indicates that “official” proclamations on the question of historical method were hardly objectivist, the responses the report elicited when it was published in 1946 demonstrate just as clearly the wide range of views practicing historians held, from the enthusiastic endorsement by C. Crane Brinton to the equally predictable dismissive criticism by Oscar Handlin.²⁰

The distance separating historians from objectivist currents of thought is likewise apparent in their responses to the efforts of philosophers, political theorists, and social scientists during these years to construct positive laws governing human conduct. Whereas linguistic analysis, conceptual clarification, and varieties of behaviorism and functionalism gained ground steadily in other

¹⁹ Samuel Eliot Morison, “Faith of a Historian,” *AHR*, 61 (January 1951): 263–64; compare Novick’s discussion of this address in *That Noble Dream*, 315–16.

²⁰ The quotations are taken from Novick’s discussion of Bulletin 54 in *That Noble Dream*, 387–92.

disciplines after World War II, historians generally viewed such efforts as futile. When Carl Hempel applied Karl Popper's concept of a "covering law" to history, and when Mandelbaum elaborated his own objectivist theory, few historians were persuaded. Even prominent philosophers, including Morton White, Arthur Danto, and William Dray, supported historians' efforts to defend themselves from the misconceived advances of postwar philosophy and social science.²¹

While there were some historians intoxicated by the idea of becoming scientists, and others, such as Hexter, who found equally irresistible the impulse to escape presentism by flinging themselves wholly into the past, many of the best American historians tried to strike a balance between their responsibility to the facts of the past and their responsibility to the issues of the present. Novick contends that, by the early 1960s, there were "signs of restlessness with what some saw as the excessively antiseptic character which an overreaction to the perceived partisan excesses of a previous generation had brought to historiography." He offers as evidence John Higham's essay "Beyond Consensus: The Historian as Moral Critic," (1962), which Novick suggests presented a model of "historiography as moral connoisseurship." But Higham explicitly called for social and political criticism of the sort Novick finds lacking among historians; Higham wanted historians not to ignore such analysis but to move beyond it by becoming more self-conscious of their commitments and refusing to apply static and simplistic political formulas to the study of the past. His position, like those of Robinson, Beard, and Becker before him, was pragmatic rather than straightforwardly objectivist or relativist—even though he criticized pragmatists (somewhat unfairly, at least in the case of James and his followers) for concentrating too heavily on social engineering and ignoring the felt experience and intentions of others. In Higham's words, "the obligation of the historian to become a moral critic grows out of the breakdown of ethical absolutes. If no single ethical system, even a pragmatic one that trusts the piecemeal results of history, does justice to all situations, a complex awareness must take the place of systematic theory." That complex awareness was the hallmark of the most gifted historians of the immediate postwar years, historians whose work cannot fairly be said to reflect what Novick calls "a deep and widespread conviction that the profession was moving steadily in the direction of establishing objective historical truth."²²

Neither Curti nor Higham—nor even Morison—endorsed the doctrine of

²¹ For this controversy, see the essays collected in *Philosophy and History: A Symposium*, ed. Sidney Hook (New York, 1963); for Novick's perceptive discussion, see *That Noble Dream*, 392–98. Recent contributions to this debate may be found in *History and Theory*, Beiheft 25: *Knowing and Telling History: The Anglo-Saxon Debate* (1986). On the rise and fall of positivism in philosophy and social science, compare David Miller, "Linguistic Philosophy and Political Theory," in David Miller and Larry Siedentop, eds., *The Nature of Political Theory* (Oxford, 1983); Jeffrey Stout, *Ethics after Babel* (Boston, 1988); and especially Richard J. Bernstein, *The Restructuring of Social and Political Theory* (New York, 1976).

²² John Higham, "Beyond Consensus: The Historian as Moral Critic," in Higham, *Writing American History: Essays on Modern Scholarship* (Bloomington, Ind., 1970), 155–56; compare Novick, *That Noble Dream*, 375–77, 362.

historical objectivity or even adopted what Novick terms a strategy of “rejection by partial incorporation.” Further examples could be added from Novick’s discussion of the historiography on slavery and the Civil War, which he tries unsuccessfully to fit into his periodization as evidence of the prevailing consensus surrounding the desirability of objectivity. New values played at least as large a part in the new interpretations of these issues as did new information. As Novick admits, Howard K. Beale, Kenneth Stampp, and David Potter all acknowledged that historians’ judgments on these highly charged questions inevitably reflect their personal convictions and their own historical contexts. Yet, as Potter argued, that awareness differed from simple-minded relativism, because steadily accumulating factual material “narrows the alternatives between which controversy continues to rage.”²³ Not surprisingly, Richard Hofstadter most eloquently expressed the position of many in his generation in the conclusion to his study of Frederick Jackson Turner, Charles Beard, and Vernon L. Parrington, *The Progressive Historians* (1968). To his credit, Novick quotes much of this passage, even though it contradicts his argument concerning the appeal of objectivity for historians in the postwar period:

[A]s one looks at the productive historiographical arguments of the past two decades, one cannot fail to see that historians are responding in their own way to the sense of crisis that is so pervasive in our time. Here the issue is an old one: they are troubled about their own role and function, caught between their desire to count in the world and their desire to understand it . . . The great fear that animates the most feverishly committed historians is that our continual rediscovery of the complexity of social interests . . . may give us not only a keener sense of the structural complexity of our society in the past, but also a sense of the moral complexity of social action that will lead to political immobility. Since a keen sense of history begets a feeling of social responsibility and a need to act, this is not necessarily the case; but history does seem inconsistent with the coarser rallying cries of politics. Hence I suppose we may expect that the very idea of complexity will itself come under fire once again, and that it will become important for a whole generation to argue that most things in life and in history are not complex but really quite simple. This demand I do not think the study of history can gratify.²⁴

Hofstadter was right about much of what passed for radical history in the 1960s and 1970s. As Novick demonstrates, many “New Left” historians, to use a label that obscures as much as it clarifies, were anything but radical in their sometimes unself-conscious and sometimes defiant commitment to a naïve objectivist epistemology. Other scholars committed to ethnic, feminist, or public history either implicitly or explicitly challenged the ideal of objectivism by denying that any “universal” vantage point exists for writing history. Some of these historians adopted the perspectivalism about truth that seems necessarily to accompany such claims. Others, Novick remarks, presumed to write “objective” histories from explicitly privileged positions because of their race, ethnicity, gender, or other characteristics. As Novick implies, such historians’

²³ Novick, *That Noble Dream*, 410; David Potter, “The Literature on the Background of the Civil War,” in Potter, *The South and the Sectional Conflict* (Baton Rouge, La., 1968), 147, quoted in *That Noble Dream*, 359.

²⁴ Richard Hofstadter, *The Progressive Historians: Turner, Beard, Parrington* (New York, 1968), 463–66; compare Novick, *That Noble Dream*, 407.

minds were clearly too broad to be troubled by the hobgoblin of logical consistency.

NOVICK SHIFTS HIS GROUND QUICKLY when he turns his attention to the rise of public history. He dismisses as “opportunistic relativism” the forthright claims by some historians that their admitted advocacy need not compromise their scholarly integrity. In a book dedicated to demonstrating the futility of efforts to escape the historicity of historical writing, that criticism seems jarring. Novick’s treatment of one of the most controversial cases of this decade, the battle over Rosalind Rosenberg’s and Alice Kessler-Harris’s testimony in the case of *Equal Employment Opportunity Commission vs. Sears, Roebuck & Co.*, is equally puzzling. After having repeatedly chided historians for their timidity in hiding behind the claim of scholarly detachment for most of the twentieth century, Novick concludes, “Of all the illusions in which we seek refuge, none is more pathetic than that which holds out the prospect of satisfactorily resolving irreconcilable claims [between scholarship and partisanship]. In such circumstances, we cannot steer between, but rather ricochet off, the rocks on either side of the channel, inevitably getting a bit bruised in the process.”²⁵ Lurking behind this elegant image, unfortunately, is the “Cartesian anxiety” of the grand “Either/Or” imposed by Novick’s dualistic conception of objectivity and relativism. Even Max Weber, whose essays “Science as a Vocation” and “Politics as a Vocation” remain the *locus classicus* for such discussions, entertained at least the possibility of scholarship helping individuals clarify the meaning of their partisan commitments. While there may be no ultimate resolution of this conflict, Novick’s stark, Nietzschean formulation of the dilemma is less attractive, and also less consistent with the position of pragmatic hermeneutics, than the anguished but resolute stance Weber chose to adopt. The conflict between commitments to historical study and political action can certainly be poignant, and its results can be tragic, yet despair is hardly the only response to that realization.²⁶

Inevitably, many historians will be especially interested in Novick’s account of another widely discussed controversy in the historical profession during the last decade, the David Abraham case. To his credit, Novick makes no pretense to neutrality, noting at the outset that Abraham “was my student, and is my good friend.” Novick contends that the “hyperobjectivism” of historians reacting against postmodernist challenges to empiricism lay behind the “persecution” of Abraham. After previously having been denied tenure at Princeton University,

²⁵ Novick, *That Noble Dream*, 516, 510. For a more comprehensive treatment of the *Sears* case and Rosalind Rosenberg’s role in it, see Thomas L. Haskell and Sanford Levinson, “Academic Freedom and Expert Witnessing: Historians and the *Sears* Case,” *Texas Law Review*, 66 (1988): 1629–59.

²⁶ Weber’s essays are most accessible in *From Max Weber: Essays in Sociology*, eds. and trans. Hans Gerth and C. Wright Mills (New York, 1946), 77–156. Unfortunately, there are problems with the translation of these essays that have led to considerable confusion concerning Weber’s meaning, a problem I have discussed in *Uncertain Victory*, 340–42, 497 nn. 92, 93. See also Wolfgang Schluchter’s analysis of these essays in Schluchter and Guenther Roth, *Max Weber’s Vision of History: Ethics and Methods* (Berkeley, Calif., 1979).

Abraham was denied employment elsewhere, at least in part because of factual errors in his neo-Marxist study of Weimar Germany, errors discovered by other historians and later admitted by Abraham himself to be "inexcusable." Whether or not this is a case of academic freedom or even a case turning on the question of objectivity are questions historians will have to answer for themselves. Novick offers the two most plausible readings of the entire sad affair: "Professional historians might sympathize with Abraham," he writes, "but found it awkward and embarrassing to defend one who had confessed to numerous 'inexcusable' errors, let alone hire him." Alternatively, Abraham's champions were more upset by the tactics of his critics, notably Gerald Feldman and Henry Turner, than they were by Abraham's own mistakes. In their view, according to Novick, "the 'bottom line'—Turner and Feldman triumphant, and Abraham out of the profession—was a striking demonstration of the continued power of the empiricist-objectivist alliance."²⁷

Novick discusses recent challenges to that alliance in a chapter called "The Center Does Not Hold." This may prove the most valuable part of the book for many historians curious about contemporary critical debates but unable to keep up with rapidly shifting fashions across the wide range of scholarly disciplines. Novick moves sure-footedly through the quicksand of postmodernism, arguing that scholars from a variety of backgrounds are converging toward the position that objectivity is an illusion. "Most crucially, and across the board, the notion of a determinate and unitary truth about the physical or social world, approachable if not ultimately reachable, came to be seen by a growing number of scholars as a chimera." The principal contributors to this convergence were T. H. Kuhn, who confessed in *The Structure of Scientific Revolutions* that "the notion of a match between the ontology of a theory and its 'real' counterpart in nature now seems to me illusive in principle"; Michel Foucault, who demonstrated the ubiquitous alliance between power and systems of knowledge; the philosophers of anti-foundationalism, Richard J. Bernstein, Nelson Goodman, Hilary Putnam, and Richard Rorty; and literary critics such as Jacques Derrida, Stanley Fish, and Edward Said. All of these diverse thinkers challenged assumptions of determinate referential meanings and advanced various—and hardly compatible—theories concerning the construction of meanings in linguistic codes by human communities and traditions. Novick also surveys relevant developments in the social sciences, paying particular attention to the influential writings of Clifford Geertz on interpreting cultures, Paul Ricoeur and Charles Taylor on the inescapability of hermeneutics, the rise of critical legal studies (the ambiguous epistemological assumptions of which he does not probe very deeply), and finally recent developments in psychoanalysis that conflict with Sigmund Freud's own conception of his "science."²⁸ When Novick steps back to assess the significance

²⁷ Novick, *That Noble Dream*, 612–21.

²⁸ Novick, *That Noble Dream*, 523; T. H. Kuhn, *The Structure of Scientific Revolutions*, 2d edn. (Chicago, 1970), 171; Novick discusses Kuhn on 526–35 of *That Noble Dream*. On the epistemological uncertainties of critical legal studies, see Joan Williams, "Critical Legal Studies: The Death of Transcendence and the Rise of the New Langdells," *New York University Law Review*, 62 (1987): 429–96.

of these critics of objectivism, his grip is, understandably, less sure. He points out that defenders of traditional realist epistemologies have had no new ideas to offer in response to the barrage of postmodernism. "It is no criticism of the defenders of traditional epistemologies," he concludes blithely, "whether of the left or of the right, to note that they offered very few new arguments for . . . the objectivist program." Most, he notes, simply cried "Back to Popper!" and left it at that. Announcements of the death of objectivism, Novick realizes, are premature.²⁹

THERE IS AN ALTERNATIVE to the categories of objectivism and relativism. Novick acknowledges its existence, but he rejects it so abruptly that he leaves readers with a bleak assessment of history's prospects in a world in which all knowledge claims are greeted with suspicion. His pessimism seems to me unwarranted. The profession is bursting with the new ideas and fresh energy of historians who represent groups previously excluded from academic life, historians who explore the lives of people previously ignored by those who thought they could impose their own views of what mattered in the past. The fragmentation of the profession into numerous subspecialties makes life more difficult for historians, but the resulting proliferation of historical knowledge wonderfully broadens and deepens our appreciation of the diversity of human experience. Poststructuralists have made us aware that telling any story prevents us from telling another story that might be told as well, but the resulting self-consciousness about our responsibility to exercise judgment may at last doom the simple-minded objectivist myth. Novick is less confident about this outcome. He offers only a brief discussion of the alternative that is attracting a growing number of scholars, reliance on "communities of the competent" to advance knowledge—even if those communities must constantly be expanded and those advances are only provisional. In dismissing this strategy, and particularly Rorty's version of it as "the conversation that is us," as just another form of self-satisfied "bourgeois mystification," Novick slides over a crucial distinction between C. S. Peirce's ideas and those of James and Dewey. Whereas Peirce sought to find a logic of inquiry that would eventually but inevitably yield Truth, James and Dewey placed their confidence in free-floating communities of inquiry whose results could never achieve that status. That confusion may also explain why Novick, in his concluding chapter, "There Was No King in Israel," so puzzlingly underestimates the importance of recent writings by Thomas L. Haskell and David A. Hollinger, which he characterizes as nothing more than a futile effort to "stake out an epistemological 'vital center.'"³⁰

Both Hollinger and Haskell have argued for the value of knowledge that emerges from communities of inquiry, knowledge that is rooted in nothing more

²⁹ Novick, *That Noble Dream*, 568–69. Allan Megill discusses this lingering positivism in "Recounting the Past: 'Description,' Explanation, and Narrative in Historiography," *AHR*, 94 (June 1989): 632 n. 19.

³⁰ Novick, *That Noble Dream*, 571–72, 628.

stable, but also nothing less precious, than the contested traditions that constitute our culture and give us our moorings. More in the Deweyan spirit of Bernstein than in the increasingly Derridean spirit of Rorty, Hollinger and Haskell have endorsed what Haskell calls "the moderate historicism that . . . provides a safe haven" for principles that our culture will continue to cherish even though we now realize the metaphysical ground has been pulled out from under us. Our convictions can be rooted in conventions rather than Truth and still have important consequences, as Haskell has demonstrated brilliantly in the pages of the *American Historical Review*.³¹ Novick's allusion to Arthur Schlesinger, Jr.'s *Vital Center* (1949) seems more than casual, since he explicitly likens Hollinger's efforts to "those of the postwar historians who had dealt with the interwar relativist critique by a strategy of 'restriction through partial incorporation.'" Here, too, Novick misses the point of the pragmatic hermeneutics that characterized the work of earlier historicists and that marks the work of Haskell and Hollinger as well. They are not auditioning for "the role of centrist statesman" he reserves for them, nor are they merely "seeking to neutralize the most radical implications" of postmodernist thought.³² They are instead trying once more to convince American historians that there is an alternative to the sterile dichotomy between objectivism and relativism. Their position is neither old-fashioned realism nor new-fangled nihilism. As Haskell has written,

There is fear in some quarters that by assigning convention and history such a large role in moral thinking, we open the door to all the worst excesses of the neo-Nietzscheans. In my view, that fear is misplaced. By mapping more precisely the pale beyond which morality is irredeemably historical, we do concede some territory to the criterionless wilderness and bring a regrettable measure of satisfaction to the radical wing of historicism. But we also demarcate a spacious domain within which rights and other claims of objective moral knowledge can enjoy something like "universal" sway. That historically defensible sense of objectivity, that *provisional* immunity to incursions of time, place, and circumstance, is all we can realistically hope for. More important, it is also all we need.³³

Despite Novick's pessimistic assessment of the chances that anyone will listen to Hollinger's and Haskell's ideas, and despite his confident proclamation that, "as a broad community of discourse, . . . the discipline of history [has] ceased to

³¹ Thomas L. Haskell, "The Curious Persistence of Rights Talk in the 'Age of Interpretation,'" *Journal of American History*, 74 (1987): 986; see also Thomas L. Haskell, "Capitalism and the Origins of the Humanitarian Sensibility, Part 1," *AHR*, 90 (April 1985): 339–61; Haskell, "Capitalism and the Origins of the Humanitarian Sensibility, Part 2," *AHR*, 90 (June 1985): 547–66; Haskell, "Convention and Hegemonic Interest in the Debate over Antislavery: A Reply to Davis and Ashworth," *AHR*, 92 (October 1987): 829–78; and Haskell, "Professionalism *versus* Capitalism: R. H. Tawney, Emile Durkheim, and C. S. Peirce on the Disinterestedness of Professional Communities," in Haskell, ed., *The Authority of Experts: Studies in History and Theory* (Bloomington, Ind., 1984). Many of Hollinger's essays have been collected in David A. Hollinger, *In the American Province: Studies in the History and Historiography of Ideas* (Bloomington, Ind., 1985). I have discussed Hollinger's pragmatic hermeneutics in more detail in James T. Kloppenberg, "Deconstruction and Hermeneutics as Strategies for Intellectual History: The Recent Work of Dominick LaCapra and David Hollinger," *Intellectual History Newsletter*, 9 (1987): 3–22. For LaCapra's response to my criticism of his deconstructionist historical method, which I find intriguing and important but less fruitful than Hollinger's and Haskell's, see *Intellectual History Newsletter*, 10 (1988): 3–10.

³² Novick, *That Noble Dream*, 626.

³³ Haskell, "Curious Persistence of Rights Talk," 1008.

exist,” it is possible to see glimmerings of a morally alert moderate historicism elsewhere in the profession—and not only on its margins.³⁴ In his eloquent AHA presidential address of 1976, Gordon Wright unashamedly invoked the model of Jean Jaurès as a scholar and activist whose writings and life reflected the social democratic values he embraced. Wright further endorsed the historian’s duty to commit what David Hackett Fischer has called “the moralistic fallacy.” “For some of us, at least,” Wright suggested, “our search for truth ought to be quite consciously suffused by a commitment to some deeply held humane values. The effort to keep these two goals in balance may be precarious; but if we can manage it, perhaps we will be on the way to re-establishing the role of history as one—and not the least—of what we might fairly call the moral arts.” While Wright discarded the Victorian notion of the “moral sciences” just as he discarded the Victorian notion of history as a science, he preserved the conviction that beyond relativism and objectivity lies a fertile field that historians can cultivate in their work.³⁵

Likewise, William H. McNeill called on historians to attend to all the complexities of world civilization in constructing their “mythhistories.” Even if we concede that the objectivist dream of “eternal and universal Truth about human behavior is an unattainable goal,” we can still work to make our versions of the past truer because they are more comprehensive, more multidimensional, more frankly tentative in tone, and more sensitive to the diversities of human cultures than our predecessors’ accounts have been. In his conception of history, McNeill, like Wright, moved beyond the dichotomy between facts and beliefs. He combined a Jamesian will to believe in the possibility that mythistory can motivate a culture to live up to its collective aspirations with a Deweyan commitment to social action: mythistory, in his words, can be “a useful instrument for piloting human groups in their encounters with one another and the natural environment.”³⁶

³⁴ Novick, *That Noble Dream*, 628. On this issue, see also John Toews, “Perspectives on ‘The Old History and the New’: A Comment,” *AHR*, 94 (June 1989): 697–98. Reflecting on the disagreements between Theodore S. Hamerow and Gertrude Himmelfarb on the one hand, and Lawrence Levine and Joan Wallach Scott on the other, Toews argues that “even among opposing representatives of the new and the old . . . there persist significant areas of continuity and agreement in theoretical assumptions and moral values . . . The point is that [these historians] do not articulate incommensurable perspectives but arguable positions in a meaningful dialogue. They are part of a historical discourse about the nature of the past and its relationship to the present that possesses significant elements of continuity with the often conflicting but sometimes complementary relationship between two traditional master narratives connecting memory and hope that can be traced back at least as far as the eighteenth century: the story of the struggle for emancipation and the story of the struggle for social integration and social unity. The expansion of these stories to include those previously defined as strangers in our midst or ‘others’ beyond our borders will undoubtedly entail the production of more complex interactive and open-ended narratives, but to equate such expansive revisionism of the stories of liberation and inclusion with the collapse of historical coherence is simply to assume the universality of the perspective of those for whom liberation is grounded on domination and citizenship entails pledge and exclusion.”

³⁵ Gordon Wright, “History as a Moral Science,” *AHR*, 81 (February 1976): 1–11; Fischer, *Historians’ Fallacies*, 78–82.

³⁶ McNeill, “Mythistory,” 8, 10.

IN HIS CONCLUSION, NOVICK laments that by the 1980s, "there was no king in Israel," and, as a result, "every man did that which was right in his own eyes."³⁷ But, for American historians, the prospect seems less bleak. We need not choose between the absolute authority of facts and the anarchy of idiosyncratic interpretations, as Novick's biblical image suggests. We can, of course, deny, as Novick does, that any alternative exists to the equally indefensible positions of objectivism and relativism. But we can also choose to continue testing the pragmatic value of moderate historicism, an option that has appealed to historians from Robinson and Becker to Wright and McNeill. This approach acknowledges both the indispensability of the scientific method of verifying facts and the equally indispensable hermeneutic method of interpreting the meanings of the past we seek to explain.

Some readers will doubtless find my perspective, which a friendly critic has termed "Dewey-eyed," at least as problematical as the old scientific history or the newest radical historicism. They may be right. One of the most important contributions of Novick's book is its demonstration, at the level of simple empirical fact, that American historians over the course of the last century have changed their minds about many crucial questions, sometimes because of new information but often simply because of new ways of thinking. By compiling and skillfully presenting this mountain of evidence, Novick has made more difficult positivist protests about the sovereignty of facts in a historical discourse that has reflected so clearly the shifting values of a changing culture. Historians have not simply shown the past "wie es eigentlich gewesen," as Ranke insisted they should, nor have they shown clearly "the events which happened in the past," as Thucydides claimed to do. As Leonard Krieger has demonstrated, historians have almost always smuggled themselves into their accounts, either explicitly by presenting philosophies of history or implicitly by providing coherence through narratives calculated to provide coherence.³⁸ American historians, Novick's study demonstrates, have done the same. Yet neither does *That Noble Dream* confirm the suspicions of relativists who doubt the reliability of everything in all historical accounts. Through a combination of imagination, technology, and diligence, historians have compiled hard data that now make impossible some versions of the past that once passed as truth. Only within this realm of the verifiable can competing interpretations survive the scrutiny of the community of professional historians.³⁹

³⁷ Novick, *That Noble Dream*, 628.

³⁸ Krieger, in *Time's Reasons*, 107–36, explained why the historicism that emerged in the late nineteenth century constitutes the notable exception to this general rule.

³⁹ While deconstruction can be a powerful tool for critics interested primarily in the indeterminacy of meanings conveyed by texts, its utility for many historians is more limited. I have discussed in some detail my misgivings about deconstruction in "Deconstruction and Hermeneutics as Strategies for Intellectual History." While some historians continue blithely to recommend that their colleagues incorporate deconstructionist insights into their work, such enthusiasts seem to me to have misunderstood Derrida's point. If he is correct, we cannot do history any more than we can do philosophy. We should simply concede the primacy of rhetoric over logic and become poets, as Derrida himself has done over the last decade. Historians longing to exercise their creative urge will find such an option attractive, while those who conceive of history as a social practice rather than an exercise of the imagination may find it less compelling. Derrida himself has recently illustrated,

It is precisely because the indeterminacy of truth and the historicity of reason are now widely conceded that we can no longer claim to find objectivity—in science or in history. It is, furthermore, precisely for that reason that historians must insist on the indispensability of historical studies as one of the most fruitful forms of inquiry in a world of uncertainty. We cannot have, nor should we want, the self-righteous smugness of earlier generations that we have “got it right” once and for all. But that should not cause us to despair about our prospects for making progress. Beyond the noble dream of scientific objectivity and the nightmare of complete relativism lies the terrain of pragmatic truth, which provides us with hypotheses, provisional syntheses, imaginative but warranted interpretations, which then provide the basis for continuing inquiry and experimentation. Such historical writing can provide knowledge that is useful even if it must be tentative. It is within that realm that historical truth—like all truth in a world that has moved beyond the discredited dualisms of both positivism and idealism—must be made, questioned, and reinterpreted. As historians, we cannot aspire to more than a pragmatic hermeneutics that relies on the methods of science and the interpretation of meanings. But we should not aspire to less.

however inadvertently, the perils of deconstruction for historical interpretation. In his essay “Like the Sound of the Sea Deep within a Shell: Paul de Man’s War,” originally published in *Critical Inquiry* and included in Werner Hamacher, *et al.*, eds., *Responses: On Paul de Man’s Wartime Journalism* (Lincoln, Neb., 1989), 127–64, Derrida probes beneath the surface of de Man’s collaborationist writings to reveal an altogether predictable contestatory subtext. Explaining (away) the most notoriously anti-Semitic of de Man’s contributions to *Le Soir*, “Les Juifs dans la Littérature actuelle,” March 4, 1941, Derrida contends that “the *whole* article is organized as an indictment of ‘vulgar antisemitism.’” Even more breathtakingly, Derrida then asserts that “to judge, to condemn the work or the man on the basis of what was a brief episode, to call for closing, that is to say, at least figuratively, for censuring or burning his books is to reproduce the exterminating gesture against which one accuses de Man of not having armed himself sooner with the necessary vigilance.” Readers of recent debates may find this rhetorical strategy familiar, since critics of deconstruction are not infrequently accused of similar (if less apocalyptic) intolerance. The misrepresentation here, however, is especially revealing. Some charges leveled at de Man have surely been intemperate and wrong-headed; it is foolish to dismiss deconstruction on the basis of de Man’s personal values. But to compare the responses of de Man’s critics with an “exterminating gesture” that became reality exemplifies precisely the blurring of texts with lived experience that has troubled radical critics of deconstruction ranging from Foucault to Habermas. Although deconstruction should survive the attacks of de Man’s critics, as a tool for historians it may not survive Derrida’s defense. For an antidote to Derrida’s creative but “antihistorical” reading of de Man’s text, see the sophisticated analysis in William Flesch, “Ancestral Voices: De Man and His Defenders,” in *Responses*, 174–84. This entire unfortunate incident powerfully illustrates why historians must consult not only texts but contexts, and not only linguistic expressions but the realities of lived experience beyond language, in their always imperfect attempts to understand and interpret the past for the present.



ROBERT A. ROSENSTONE
Contributing Editor

Why review history on film? The easy answer, “because it’s there,” may do for the converted, but those who fail to find any serious purpose in the visual media will hardly be satisfied with such a response. Nor should they be. The value of film as entertainment, as a teaching tool, even as art is easy to see. But film as history, as discourse, as a way of seriously discussing historical issues: this use of the medium can neither be assumed nor asserted—it must be demonstrated.

To give skeptics their due, the evidence, or lack of it, seems to be on their side. Historical films have been made since the early days of the medium, yet it would no doubt be difficult for the average historian to name a motion picture that makes a contribution to a field. In films devoted to entertaining mass audiences, the past has often become just one more setting for tales of adventure and romance. But most historians simply do not know much about motion pictures. We are schooled neither in the rich film traditions of the United States, Europe, Asia, Africa, and Latin America, nor in the language of film, the way in which it communicates. Without such knowledge, it is impossible for us to judge whether film can be used as a serious means of exploring the past.

Even partisans of the visual media often convey a kind of subtle skepticism about the possibilities of history on film. At scholarly meetings and symposia, in journal articles and reviews, they most often analyze films as documents that provide insights into the social and intellectual climate of the era in which they were produced. However interesting the results, such an approach provides no distinct mission for the specifically historical film. For this purpose, works of visual history are no different from any other sort of film; they may be symptoms of significant historical change, but they cannot deal meaningfully with historical issues.

One aim of this *AHR* section is to consider the contrary—to see to what extent film can be used to represent, re-create, talk about, and situate us with regard to the vanished world of the past. Put another way, our purpose is to grapple with the question of what we would want history on film to be if we wanted to put history on film. Since both history and film are, in part, art forms, this question cannot very well be answered in the abstract. Our approach will be to have reviewers look at a variety of films and to assess if and how they work as history. This means not just to analyze content but also to assess form, not just to evaluate data but also the vehicles with which that data is delivered.

Recent developments make the moment right for serious consideration of film as a way of thinking about the past. Fueled by the money of various Western governments, networks, and independent foundations; propelled by the needs of women and ethnic minorities for self-definition, and of Third World peoples to recapture the past from colonialist methods of representation; and aided by the growing complexity in both the theory and practice of the ethnographic film, filmmakers around the world—sometimes helped by historians—are increasingly creating works worthy of attention from those who care about history. At the same time, the rise of powerful critiques of representation

(which highlight the constructed nature of our knowledge of the past) provides us with a way of removing the ideological blinders that insist history must be constituted in words. Finally, the decreasing cost of video technology makes the creation of visual works by historians themselves ever more possible and likely—and this development in turn makes it ever more imperative for us to consider standards by which to judge such works.

What films to choose for review in the *AHR*? Constraints of space and time compel a limit of only a dozen or so film reviews a year, which is a tiny percentage of historical films made in the United States during that period, let alone the world. As a result, each work must stand for more than itself; each must be an example of a possible approach to history on film, as well as a vehicle of some specific knowledge. With these limits in mind, I chose films not just on the basis of geographic diversity but also on the grounds of their quality, seriousness, and the inventiveness of their historical strategies. Included are films from five continents—some made with high budgets and others low, some standard in their film language and others experimental, some dramatic in form and others documentary.

Those scholars generous enough to undertake to do reviews were given a difficult task—nine hundred words with which to evaluate the content and form of complex texts. The specific approach was left up to the individual, although written guidelines suggested the following: that they consider the question of what we expect a historical work to tell us; think about history on film in its own terms, rather than as a way of illustrating a preexisting written text; focus on major themes rather than minor details; stress the specific contributions of the visual and aural elements toward historical understanding; point to things that the film can do that written history cannot; describe elements of the film that suggest anything about the broad possibilities for visual history; and refuse to feel bound by preconceived notions of either what history is or what a review must do.

The results are as varied and interesting as the works they cover. Some reviews focus on historical content, others on film language; some on the history outside the film, others on the data delivered on screen; some on thematic statement, others on its reception. What they have in common is an acknowledgment of an important notion: motion pictures do not provide a transparent window onto the world of the past. Film is, like the written word, a medium that constructs the past and its meaning—in this case, by utilizing images and sounds in addition to language.

To take up the broader issue—the extent to which the visual media can create a past worthy of serious consideration—I suggest we look at films on their own terms, as documents written in a visual language, as arguments about the meaning of the past that proceed by different rules than does written history. To do so is to rethink some of the familiar attacks on the historical film. Consider, for example, the common charge made against the dramatic film: that its (apparent) need for individual exploits, romantic attachments, and a compelling plot inevitably simplify and falsify history. Put aside the fact that the model for such a notion is the Hollywood historical romance, put aside that one can make a somewhat similar charge against all narrative history (for instance, it has tropes that shape its meaning), put aside that history must come to us in some form that will inevitably help to determine the meaning of its content, and test this notion against the dramatic works reviewed in this issue.

To do so is to find the dramatic film neither a simple form nor one that must necessarily falsify history. Covered here are six works that structure and comment on the past in a wide diversity of ways. According to our reviewers, the drama can successfully do the following: render conflict in a medieval village; tell the story of an artist, a radical, or a political adventurer; underscore political continuities between the past and the present; argue strong feminist or anti-colonialist theses; raise questions about the very nature of history. For those interested in the potential of the medium for expanding our sense of the past, the more experimental works—rather than the standard dramas—will prove most interesting, for their modes of presentation point toward new sorts of visual histories. When an African filmmaker chooses to use that traditional bearer of oral

history, the *griot*, to recount events; or when the biography of a Mexican artist is told largely in fragmentary images; or when the story of a democratic American imperialist is rendered in a postmodernist style full of outrageous humor and blatant anachronisms, we enter a realm in which our usual notions of historical genre, evidence, argument, proof, and meaning must be rethought and revisioned.

To move from the dramatic film to the documentary is to enter a realm in which form seems to be less important than content. But this difference is more apparent than real. The word "documentary" is itself a misnomer, at least if the implication is that the camera can somehow document a passive past. As much as the drama, the documentary presents a world that has been shaped by the filmmaker. Those reviewed here use a number of ways to create that world. Some attempt to show us events and explain them; others invite us along on a search to find out what happened and why; still others create history in the specific sense of using the camera to cause things to happen. No matter what the strategy, the documentary usually argues a thesis motivated by moral concerns. Much—perhaps all?—written history does the same, but, unlike the monograph, the documentary rarely works to hide its rhetoric.

The most typical documentary is that which reconstitutes history in a narrative about a person, a movement, a campaign, an era; the most typical technique (at least in recent years) is to stitch together images from the past (moving and still) with an argument made by either an unseen narrator or talking heads. Some recent films move beyond this approach and attempt to create a more complex, less linear, more open-ended historical world. One way to do this is to go beyond the method of illustrating words with old images (someone talks about the Great Depression and we see Hoovervilles) by raising questions of both why those particular images were shot and how they were used in the past to create filmic worlds. This approach does more than provide another historical layer—it points to the importance of context in creating all meaning, including that of the images used to make such an argument. Another strategy is to deal less with the past as event than as process, to be less concerned with pinning down exactly what happened and more with how what happened means to us today. Works that search for the past often take this form, sharing with viewers conflicting and contradictory historical evidence as well as the filmmaker's own moral dilemmas over the issues of the past.

This introduction and the reviews that follow should make it clear that the films here have been chosen with a bias for works that push at or stretch beyond the usual boundaries of form and genre. Such films raise more difficult questions about the nature of history than do the standard dramatic feature and documentary. They also seem consonant with the mission of the *AHR*. This journal does not generally review popular written histories but confines itself to works serious in methodology, content, and intent. The same policy must apply to film. That the best historical films will be different from written works is obvious. They will make their arguments differently, and they will deliver different sorts of data. Yet the difference of medium need not prevent films from being as complex, intellectually stimulating, rigorous, and serious about the meaning of the past as any written work. Only by studying the kinds of films reviewed here, the most demanding examples of history on film, will we be able to assess the potential of the visual media for broadening our understanding of history: only with such study will we begin to confront films that we as historians dare not ignore if we are to understand more fully how the visual media can broaden the dimensions of our relationship to the past.

AFRICA

Jom, ou l'histoire d'un peuple [Jom, or the Story of a People].
 Directed by Ababacar Samb. 1981; color; 80 minutes.
 Wolof with English subtitles. Distributor: New Yorker
 Films.

An unusual blend of epic and social history that is based on fact, *Jom* conveys a message from the past to modern Africa. The film was made in 1981, the third and last work of the well-known Senegalese director, Ababacar Samb (president of the African Filmmakers Federation), who died in 1988. In this work, Samb continues a celebration of traditional values that he initiated in earlier films. Well-served by superb photography and beautiful traditional music, *Jom* takes its own definition as its theme. In Wolof, the language of Senegal's dominant ethnic group, "jom" means "dignity and pride"—the film's story serves as an illustration and a defense of this value over some seventy years of Senegal's history. The form of the work also serves to carry out tradition. History here is described by a traditional historian and storyteller, a *griot*, who uses the events surrounding a contemporary factory strike to recall the lessons and meaning of two earlier historical episodes.

The first, known as "The Thiès Incident," occurred in 1905, when a Wolof prince, Dieri Dior Ndella, killed a French administrator rather than submit himself to colonial rule. Later, when surrounded, the prince chose to avoid capture and died in dignity by taking his own life. The second episode centers on Koura Thiaw, a celebrated female artist of the 1940s who, in the then capital city of Saint-Louis, used song and dance to expose and lampoon the city bourgeoisie—especially the women—both for abusing and exploiting their rural countrywomen (working as city maids) and for distancing themselves from the indigenous culture.

The historical flashbacks, as called up by the *griot*, interweave with the continuing events of the strike at the factory. Despite the pressures brought to bear on the workers, including attempts at bribery, most stand firm. They are finally aided by their wives, who invade the work site and remove all who have served as strikebreakers. The message here is clear: masters may change from Europeans to Africans, historical actors from princes to simple factory workers, motives from political independence to economic aspiration, but "jom" continues as the untouchable, transcendental central value of Senegalese society and culture.

Appealing as the film's theme may be, its use of history is not entirely effective. The story of Prince Dieri, for example, seems much more convincingly rendered than that of Koura Thiaw. This may be because the *griot* seems more comfortable describing the exploits of royalty rather than those of an entertainer or because a warrior's suicide seems more closely linked to the physical struggle of a strike than do the song and dance activities of Thiaw. A greater problem concerns "The Thiès Incident." Because it was sparked by the French fight against slavery, its use here leaves the uneasy impression that in supporting one traditional value the film defends another, highly dubious one.

Clearly, one of Samb's aims in *Jom* was to use Wolof culture to draw in his audience. But to do so was to pursue a less than happy particularist strategy in a nation of plural cultures like Senegal. Even more problematic was the timing of the production. The official motto of the shaky government of President Senghor in 1980 (he resigned at the end of the year) was "Jom, Kersa, Mun" (dignity, restraint, patience). Appearing just at a moment when Senegalese society was tired of official history that utilized traditional values to bolster political power, *Jom* was thus—against the author's intentions—perceived as a defense of this unpopular, outgoing regime. Here is an ironic lesson. The reception

of this film underscores the fact that the meaning of any work of history, filmed or written, is always subject to the receptivity of the particular audience, a receptivity over which the author has no control.

This is a fate that history on film shares with other forms of recalling the past. Like written and oral history, a work on film such as *Jom* is worthwhile as long as the message it delivers is not used to obfuscate or distort the facts. We must thus always be concerned about the motivations, accuracy, and credibility of those who undertake to do history. Like all history, that contained in the film *Jom* has no intrinsic value apart from the uses we wish to make of it.

Mohamed Mbodji

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ASIA

The Last Emperor. Produced by Jeremy Thomas; directed by Bernardo Bertolucci. 1987; color; 166 minutes. Distributor: Columbia Pictures. Video: RCA/Columbia.

Bernardo Bertolucci spent 25 million dollars making *The Last Emperor* and won nine Oscars for his effort, but historians of China, with few exceptions, refuse to take this lavish production very seriously. Among other things, they object to the invention of some episodes, such as Pu Yi's attempted suicide in 1950, and the inexplicable omission of genuinely important moments in his life, such as his five-year imprisonment in the Soviet Union in the late 1940s.

But a more serious difficulty is that the story is based primarily on the notoriously unreliable "official autobiography" published by the Chinese government in 1963 after Pu Yi had undergone nine years of "reform through labor" in a Chinese prison for "war criminals." The film, like the autobiography, views the last emperor's life through Communist party-approved lenses. Of course, Bertolucci had no choice, since some form of government script control is normally a precondition for filming in China. In fact, Li Wenda, the man who "helped" Pu Yi write his autobiography nearly thirty years ago, was brought in to advise Bertolucci. *The Last Emperor* demonstrates just how risky it is to base visual accounts of history on a written text, especially when the text is seriously flawed.

But why not give Bertolucci the benefit of the doubt by treating *The Last Emperor* as a work of fiction and ignoring the sort of embellishments one expects to find in commercial films? As a fictional account, does it offer any interesting insights on the social, economic, cultural, and political problems of China in the twentieth century? The answer, unfortunately, is no. Bertolucci, unlike the producers of *Reds* (1982), does virtually nothing to locate the story of an individual life in a meaningful historical context. The film is trapped by its subject, a powerless and pathetic pawn whose life is irrelevant to the central issues of his time. The problems that preoccupy historians—the 1911 revolution, political and social currents, the May Fourth Movement, the Nationalist-Communist alliance, the Nanjing decade, the rise of the Communist party during World War II, and the Cultural Revolution—are never introduced in a coherent way. The audience, like the emperor himself, is kept in the dark about events in China. There is only interminable chaos and confusion.

The Last Emperor is biographical in the narrowest sense of the word. It is an account of Pu Yi's lifelong imprisonment that manages to imprison the audience by never allowing the viewer to see the complex and dynamic Chinese world that lies beyond the

Forbidden City in Beijing, the imperial residence in Changchun and the labor camp in Fushun. The contribution of the film is limited to a portrait of an emperor who has no realm and is manipulated first by aging imperial consorts and corrupt eunuchs, and later by Japanese militarists and Communist revolutionaries.

Bertolucci had a chance to break new ground on the truly interesting issue of the relationship between Pu Yi and the Japanese in the 1925–1945 period, but his treatment of the Japanese amounts to little more than a recasting of the crude, one-dimensional caricatures of Japan that have appeared time and again in Chinese films since 1937. The subject of Communist labor camps offered Bertolucci another opportunity to make a contribution, but his representation shows conditions that are far better than the ones that existed in the network of hellish labor camps that were so busy in the 1950s.

Although *The Last Emperor* has virtually nothing significant to say about the major issues in modern Chinese history, the first portion of the film, which begins when the three-year-old monarch enters the Imperial Palace in 1908 and ends when he is forced out in 1924, twelve years after his abdication, is both visually stunning and of considerable ethnographic interest. Bertolucci did what Chinese filmmakers have never been allowed to do, that is, go inside the Forbidden City to re-create in meticulous detail richly textured images of court life. These include breathtaking treatments of the arrival of the three-year-old emperor-designate in November 1908, his coronation in December 1908 and his marriage to Wan Rong in 1922. Perhaps more interestingly, they also portray things such as the emotional relationship between the boy emperor and the wet nurse who breastfeeds him until he is ten years old, and Pu Yi's use of Republican troops to expel 1,500 eunuchs (who depart with testicles in hand) from the Forbidden City in 1923.

Unfortunately, non-specialist viewers, denied any meaningful information about the historical backdrop, are likely to be attracted to these gorgeous re-creations for the wrong reasons. By electing not to introduce relevant historical themes, Bertolucci is reduced to cataloging fascinating "oriental exotica." In short, his film won nine Oscars not because it broke new cinematic ground but because it departs in no significant way from a familiar Hollywood formula that insists on treating Asia as mysterious and unfathomable.

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The Emperor's Naked Army Marches On. Directed by Kazuo Hara. 1986; color; 122 minutes. Distributor: Kino International. Japanese with English subtitles.

This documentary follows the activities of Kenzo Okuzaki, a veteran of the New Guinea fighting during World War II, who revisits, during 1982–1983, some of his wartime comrades. His main purpose is to encourage them, through verbal persuasion and sometimes even physical coercion, to remember the past. In particular, he asks them to recollect the precise details of what happened after the formal end of the war on August 15, 1945. (By then, Okuzaki himself had been taken prisoner and thus lost contact with his comrades.) Apparently, two soldiers in his regiment had been shot to death on their superior's order twenty-three days after the Japanese surrender. Why? The sister of one of the men shot wants Okuzaki's assistance, and he takes it upon himself to find out the truth.

Okuzaki, one learns, has an unusual past. He had once been jailed for manslaughter and once arrested for having fired a slingshot at the emperor. This latter incident reflects Okuzaki's belief that the emperor, as commander-in-chief, should have acknowledged his part in the war guilt.

Okuzaki's search for the truth regarding the shooting of the two soldiers is part of his quest to establish responsibility for wartime deeds. Those who do not take the blame for their own behavior are cowards, he says, and the emperor is the most cowardly of all. Much to his (and the viewer's) horror, Okuzaki discovers that, in New Guinea, surrounded by the enemy and driven to the jungle without anything to eat, the men had started feeding on human flesh; the survivors he interviews show no particular remorse as they recollect having eaten flesh from "white" (American or European) and "black" (indigenous) corpses. When these bodies were not available, however, they had taken to killing one another. That was why the officer ordered two soldiers shot. Even though the war had ended, the men had to eat, and these two were the most junior of what was left of the regiment.

The shocking incident of cannibalism is only the most obvious story of this film. Underneath is the theme, as the title suggests, of the Imperial Japanese army "marching on." Even though the officers and soldiers who survived the war and mutual atrocities are now civilians, pursuing their ordinary lives as restaurateurs, farmers, shopkeepers, and the like, their wartime memories and, even more serious, wartime attitudes remain just beneath the surface.

Okuzaki himself is no exception. His rather quaint behavior and language, his proclivity for violence, and his attitude toward men in authority (the emperor, politicians, police) whom he delights in confronting, all suggest a personality that has not experienced a normal civilian life. The war has permanently unhinged his sense of ordinary relationships to other humans. His fanaticism—at one point he says, "the use of violence is justified for the good of mankind"—is reminiscent of wartime Japanese behavior, and it is possible to see in his facial expressions, stripped of surface civilities, the same combination of brutality and self-righteousness that impressed foreign observers of the Japanese army during the war. Moreover, his self-appointed vigilante activities suggest he has not learned anything from postwar democratic reforms, which should have persuaded him to channel his anger through established political processes. Perhaps he refuses to recognize the legitimacy of such processes. His activities amount to saying that, because the nation, from the emperor on down, has not acknowledged its war guilt, postwar Japan is a sham, one that can be cleansed only by his type of one-person crusade.

All in all, this is a depressing film, but a superb text for understanding the fascinating question of the continuity between wartime and contemporary Japan, between memory and survival. Because memory is mostly visual and aural, film is an essential medium for conveying the sense of this relationship. How the haunting memories of the war still affect ordinary (and extraordinary) citizens today is an issue that seems ideally suited for the visual media. This film succeeds in creating a world in a way that no written work could possibly match.

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EUROPE

Le moine et la sorcière [The Sorceress]. Produced by Paméla Berger, Annie Leibovici, and Georges Reinhart; directed by Suzanne Schiffman. 1988; color; 96 minutes. French with English subtitles. Distributor of film and video: Lara Classics, 9 Merrill St., Cambridge, Mass. 02138.

L*Le moine et la sorcière* brings to the screen the clash between patriarchal, aristocratic, Christian values and an older, nurturing, peasant, animistic culture in thirteenth-century

France. The drama unfolds in a village: its dark smoky dwellings with their gardens and fields, the unfenced land at the forest's edge, the forest itself, are beautiful and credible. In this setting the peasants live—giving birth, working communally and individually, dying. The village, typical of its time and place, keeps a secret. Its patron saint, Guinefort, around whom the peasants have built a cult and rituals, is a heroic dog.

The rhythms of village life, the peaceful coexistence of the village cult and official Catholicism, are disrupted when the aristocratic Etienne de Bourbon, a Dominican friar, arrives to seek out heretics. Etienne's attention quickly turns to the beautiful midwife-healer Elda, but his quest is misguided. His ruthless search for heresy blinds him to Elda's nature-wisdom, just as his orthodoxy blinds him to the local lord's exploitation of the impoverished peasants. Spying on the woman, he surprises her as she conducts a healing rite in St. Guinefort's sacred grove; he destroys the site and turns her over to the secular authorities for burning. In the end, Etienne realizes his folly and reverses his decision, convincing the lord to exchange Elda's life for a relic, and ordering a chapel built in Guinefort's grove. The film's happy ending redeems the protagonist, saves the heroine, and transmutes Guinefort into an acceptably human saint, a man henceforth depicted with a dog at his side. (The real Etienne de Bourbon hinted at no such accommodation, nor did he treat such practices as heretical, in his thirteenth-century book of *exempla*.) Fusing orthodox Christianity with peasant custom, this final action restores the village's equilibrium and reveals the filmmakers' intent.

Throughout the film, the way in which the filmmakers present the twin patriarchal hierarchies of religious orthodoxy and secular dominion reflects modern concerns and ideologies as much as thirteenth-century realities. In one instance, Etienne and Elda engage in a theological argument about the orthodoxy of her reading of the book of nature (her theology, like her French, is very polished); later, they argue more seriously over her participation in the ritual in Guinefort's grove. The Dominican friar also contests the relaxed attitude of the village priest toward what he regards as the harmless superstitions of his flock. And, in a sub-plot, the lord and a peasant battle over the peasant's traditional rights to land that the lord has usurped. On the particulars of this confrontation, as on a number of significant points, the film draws on without taking full advantage of the historical nuances of Jean-Claude Schmitt's excellent treatment of the legend and rite of Guinefort, *Holy Greyhound: Guinefort, Healer of Children since the Thirteenth Century* (1983).

The seriousness with which the filmmakers take the current debates on such relationships as sorcery and the healing arts, and heresy and folk religion, adds to the film's interest but detracts from its art. Class conflict appears not only in the contrast between official and popular religion but also in the wronged peasant's revolt, as well as in Elda's own story of how she became the wise woman of the forest. A feminist perspective shapes not only the presentation of Elda's wisdom, selflessness, and beauty but also the film's emphasis on the impact of sexual violence and—in sharp contrast—its recurrent images of maternal suckling. Clearly, too, the filmmakers wish to refute a view of the Middle Ages as a time when a benevolent Christian civilization supplanted ignorance and superstition.

While the analytical frameworks that are suggested provoke reflection on historical issues, they veer too easily toward simplification. For example, the psycho-sexual ground for Etienne's zeal and crisis is contrived: in flashbacks, we learn that his dislike of fighting and hunting (requirements of an aristocratic life), elicited the scorn of his hunting companions. Fleeing their taunts, he raped a peasant maiden. Having become a friar to escape his sin, Etienne finds Elda awakening memories both provocative and unpleasant. The arrival in the village of Etienne's daughter, the product of the rape, offers only one example of the way in which we find each thread of the plot tied neatly to the others.

The film begins and ends with St. Guinefort, and its best moments concern his legend and the people who revere him. The scenes depicting the veneration of St. Guinefort evoke a strong feeling for this animistic world. Drawn in, we believe for a moment that a more perfect balance once existed between nature and humanity, celebrated in rituals that could accommodate not only birth and death, loss and renewal, but Christianity as well, if only patriarchy in all its guises would not intrude. The evolution of Guinefort's status, although represented schematically, provides the most subtle lessons the film has to offer on how the oppressed can control the oppressor, how women can name and control their lives—how, in short, the lives of thirteenth-century villagers and the arguments of twentieth-century interpreters can come together.

Katherine Anderson, Jill A. Frederick, Mary Suydam,
Jacqueline E. Robbins, Joan Cadden

Kenyon College

Rosa Luxemburg. Produced by Eberhard Junkersdorf; directed by Margarethe von Trotta. 1985; color; 122 minutes. German with English subtitles. Distributor: New Yorker Films.

Rosa Luxemburg remains one of the most powerful but contested figures in the socialist tradition. After Karl Marx and Friedrich Engels themselves, she was a leading personality of the second generation of the Central European revolutionary movement, following the grand old men, August Bebel and Wilhelm Liebknecht, Eduard Bernstein and Karl Kautsky, but coming before the post-1917 generation of Communist leaders. She was also a main voice in the radicalization of the war years that fractured the imposing solidarity of German Social Democracy into two opposing parties and one of the founders of the Spartacus League, which broke away from the Independent Social Democratic Party of Germany (USPD) in December 1918 to join with other ultra-left groups in launching the German Communist Party (KPD). Along with Karl Liebknecht, she was murdered during the repression of January 1919 and its aftermath, which accompanied the end of the first phase of the German revolution.

Luxemburg's death removed her from the scene before the rise of Joseph Stalin and the period of the most bitterly fought and destructive splits in the European Left as a whole. She was the most important of the pre-1914 revolutionaries *not* to have to face the dilemmas posed by the isolation of the revolution in Russia, although her famous disagreements with V. I. Lenin certainly provided a taste of difficulties to come. Luxemburg's legacy became an immediate object of rivalry and polemic in the Left. Communists clothed themselves in her reputation, while Social Democrats denounced her as "bloody Rosa," an incorrigible rabble-rouser who encouraged the masses in revolutionary illusions and was responsible for her own death. Later, Luxemburg's criticisms of Bolshevism led Stalin to play down her importance. It was not until the 1960s that an interest in her revived, when the New Left reclaimed her as an anti-Stalinist revolutionary and Peter Nettel published his remarkable scholarly biography (*Rosa Luxemburg*, 1966). But the element of political controversy remains.

This contested nature of its subject adds to the interest of Margarethe von Trotta's beautifully constructed, accurately conceived, and even-toned film. Moving frequently but intelligibly between different times, von Trotta presents Luxemburg's biography with great economy, splendidly assisted by Barbara Sukowa's finely measured performance in the lead role. The story is framed by Luxemburg's imprisonment during the war, moving

from the opening frames back through an earlier period of imprisonment during the 1905–1906 revolution in Warsaw to the end of 1899, when Leo Jogiches, her comrade and lover, arrived from Zurich to join Luxemburg in Berlin. The film introduces the leadership milieu and political culture of the Social Democratic Party of Germany (SPD) through the device of the party's New Year's ball on the arrival of the new century, then takes us through a series of important moments, including the mass strike debate at the 1906 Congress, the break with Kautsky in 1910, the growing threat of war in 1913, and the outbreak of war in July–August 1914. The presentation of political discussion (notoriously difficult in filmic terms) is admirably handled, much of it through Luxemburg's close personal relationships—with Clara Zetkin, Luise Kautsky, and especially Jogiches. Other close confidants—Paul Levy, Karl and Sonia Liebknecht—are introduced later during the SPD's growing internal tensions after 1910. The prison years are used to explore Luxemburg's personality and general philosophy, with a few glimpses of childhood and the use of prison correspondence. They also establish the mood for the film as a whole, where the dark domestic and public interiors are continuous with the more somber bleakness of the prison settings, qualified only by an occasional contrast (the brightness of the childhood flashbacks, a boating trip with Luise Kautsky) and the emblematic red of the socialist meetings. The film ends with the hectic days of the Spartacus rising in January 1919 and Luxemburg's and Liebknecht's murder.

Aside from the film's general excellence, three things stand out for the historian. First, the representation of the SPD is basically accurate, finely observed, and exceptionally well researched. The instances of artistic license (such as Kautsky's clown nose at the fancy dress New Year's ball or Ignaz Auer choking on his wine at Luxemburg's toast to the revolution) are apposite, and the portrayal of actual events is remarkably faithful to the record when it matters. More to the point, the culture of the German socialist movement is beautifully captured, from the orderly respectability of the leadership's existence to the atmosphere of congresses and other public meetings and the self-improving earnestness of ordinary members. Above all, the socialist tradition's late nineteenth-century ethic of progress, the sense of moving with history toward an optimistic future, is successfully communicated.

Second, von Trotta skillfully counterposes the personal and the political, dealing explicitly with Luxemburg's sexual relationships (with Jogiches until the partial estrangement after 1906, and with Kostya Zetkin), her decision not to have a child, the importance of her close female friendships, and her love of the natural world. Luxemburg is particularly interesting in what to later twentieth-century sensibilities seems a central paradox of her career. On the one hand, she was a triple outsider. Her Jewishness, her Polishness, and her gender clearly set her apart from the majority of the SPD and to a great extent disadvantaged her. On the other hand, she went out of her way to deny the importance of these issues. She was pointedly uninterested in feminism per se, and, while her career was a determined effort to show that women could join in politics on the same terms as men, it also meant ignoring specifically women's issues. Similarly, she had absolutely no time for the national question, in either its Jewish or Polish guise, and was a purist when it came to internationalism. In each respect, she was very much a person of her own time, and she has much less appeal for the politics of the 1980s, when both feminism and the national question have rightly and increasingly set the agenda. Although von Trotta appropriately draws attention to these matters, she is admirably restrained in not drawing facile contemporary morals.

Finally, the theme of war is paramount in the film. Von Trotta begins her story during the war, where the wintry starkness of the prison imparts a sense of loss and foreboding to the film as a whole. The war becomes the central destructive experience of Luxemburg's life, driving the divisions of the socialist movement between 1910 and 1918 and intervening symbolically earlier in the film, as when Luxemburg and Zetkin wander dangerously close to the crossfire of a military field exercise in 1907. It is perhaps this that harnesses the film most directly to contemporary German politics, and it can be no

accident that, when presenting the threat of war in 1913, von Trotta chooses a speech in which Luxemburg highlights the endangerment of the forty-year peace (the film was conceived and written during the fortieth anniversary celebrations of the end of World War II). The imagery of war and incarceration is mitigated mainly by the imagery of the natural world, which becomes a vital counterpoint to Luxemburg's personal and political troubles. This contrast runs throughout the film, but von Trotta uses elements of the prison letters to particularly good effect—Luxemburg's sympathy for the silent suffering and powerlessness of a harshly beaten ox, the mole burrowing to the surface even of the hard winter ground, the lovingly tended garden, the wildflower pressing, and so on. Philosophically, these themes—of war and nature—translate into a deeply felt socialist humanism whose contemporary German expression is the eco-socialism and peace commitments of The Greens (Die Grünen). In this sense, von Trotta provides a Luxemburg for our times.

Geoff Eley

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Hotel Terminus. Produced and directed by Marcel Ophuls.
1989; color; 267 minutes. French with English subtitles.
Distributor: Samuel Goldwyn Company.

Despite its subtitle—*The Life and Times of Klaus Barbie*—*Hotel Terminus* is only peripherally about the man who headed the Gestapo in Lyon and had the luck (and, in the long run, it was bad luck) to capture and execute the Resistance hero Jean Moulin. We never discover how this pious and friendly boy, who wanted to become a priest, turned into “the butcher of Lyon”; nor does the director of this film, Marcel Ophuls, seek to explain to us how a man who apparently took pleasure in the beating and murder of women and children could be remembered by his daughter, his French daughter-in-law, and his friends and neighbors in Bolivia and Peru as a kind father, sensitive musician, and soft-spoken gentleman. Indeed, Ophuls does not seem very much taken by these questions; and four-and-a-half hours of film yield little more than Barbie's laconic response to a question put to him by a journalist as he was being hustled out of Bolivia and returned to France. “Do you consider yourself a Nazi now?” the journalist asks. To which Barbie replies: “Here the word ‘Nazi’ does not exist . . . Can you explain to me what ‘Nazi’ means?”

If *Hotel Terminus* is not about the life of Klaus Barbie, what, then, is it about? The answer, I think, is memory: the way people remember their past, screening out what is unpleasant or uncomfortable, consciously or unconsciously choosing to forget what cannot be easily assimilated into the present's view of the past, or being compelled to remember because the memories are too painful to forget. An event that is a tragedy for one person may be forgotten or considered insignificant by someone else. Although we see numerous examples of these processes in the Germans, Americans, and Latin Americans whom Ophuls interviews, the primary focus is on France and the inability of the French to come to terms with their experience during the German occupation. Ophuls had already explored this theme in his earlier film *Le Chagrin et La Pitié*, but here, a decade and a half later, he returns to it and examines a people who are still very much at odds with one another over the meaning of their recent past.

To be sure, a string of distinguished historians, ranging from Robert Paxton and Michael Marrus to Zeev Sternhell, have written incisively about these issues and exposed the sinister sides of Vichy France. Ophuls has nothing factual to add to what we already know about this story. But what gives force and fascination to his film is the penetrating,

ironic, passionate, and (sometimes) compassionate eye with which he observes individual experiences, combined with an uncanny knack for uncovering an interlocking network of relationships—between Barbie and the Resistance, between Barbie and the American intelligence services, between Barbie and officials in the Vatican, between Barbie and the political elite in Bolivia and Peru—that makes one realize that the war did not come to an end in 1945. The mind boggles at the thought that, thirty years after the end of World War II, Barbie maintained an office in the Bolivian ministry of the interior and was on terms of friendship with the soldiers who ran Bolivia and Peru.

In the hands of a master like Ophuls, the documentary film has evident advantages over the written historical account. Ophuls can juxtapose images and voices separated widely in time and space in order to suggest a conclusion that the historian would have to laboriously develop, document, and possibly oversimplify by making it too explicit, an argument rather than an *aperçu*. He can move in that realm of ambiguity proper to imaginative literature and film in which there need be no “real” Klaus Barbie, merely the images he left behind in those who knew him: decent schoolboy, brutal torturer, efficient intelligence agent, loving father, generous friend, ruthless swindler, vicious anti-Semite, “noble gentleman.” And he can insert himself into his story as a protagonist, eliminating that feeling of distance between historians and their theme. Because it is composed of images, voices, and music, *Hotel Terminus* can be simultaneously about the past *and* the present in a way that few historical accounts have managed, up to now, to be.

But the price paid in historical understanding for these gains is at times dismayingly high. We never learn what Barbie believed, why he became a Nazi, how and by whom Jean Moulin was betrayed, what importance Barbie's activities in Lyon had, how extensively the American intelligence services recruited former Nazis, for what purposes they were used, and what role Barbie played in the dictatorships and drug trade of Bolivia and Peru. All these issues are discussed, but they are filtered through the memories and consciousness of people who know a fragment of the story or who speculate on the basis of information that no historian (or judge) could accept without external confirmation.

Does this really matter? From Ophuls's perspective, probably not. The truths he is after are those of individual experience and collective memory, and those can be counted upon to emerge from the techniques he has chosen to use. But the historian who admires Ophuls's art and his passionate commitment to exposing lies and bad faith, as I do, can still hope for a type of documentary that moves back and forth between individual experience, as it is remembered, and the larger tapestry of events, as we have come to know it, without sacrificing the evocative power and emotional impact that the documentary film is uniquely equipped to produce.

Robert Wohl

University of California, Los Angeles

Handsworth Songs. Produced by Lina Gopal; directed by John Akomfrah/Black Audio Film Collective. 1986; color; 60 minutes. Distributor of film and video: Third World Newsreel, 335 West 38th St., New York, N.Y. 10018.

Margaret Thatcher's Britain has become a notably riotous state. The government now frankly accepts a high rate of civil disturbance as the price of its “radical,” two-nation Conservatism. Over the last decade, the Home Office has become formidably skilled at containing disorder in the streets, whether by the Northern Irish, by students, or by picketing coal miners. In two areas, however, the Thatcher administration's response to British riot has been inept. It has failed to control mob violence by soccer fans—whether

homicidal (as at Heysel) or suicidal (as at Hillsborough). And it has found no answer to the recurrent summer race riots that have erupted in British cities since 1981.

The Handsworth outbreak took place in a working-class area of Birmingham in early September 1985. As these things go, it was nothing special. There were only two civilian deaths, some score of policemen injured, and under a hundred arrests. A bus was attacked and two passengers hurt. Twenty or so stores were attacked and systematically looted. There were some ugly acts of arson. But order was restored after three days, and the entire episode was soon eclipsed by a more serious riot in Brixton at the end of September.

There remained, however, some perplexing aspects to the Handsworth riot. The West Midlands constabulary is noted for its "sensitive" community policing. The officers are not, like their more racist colleagues in Brixton, notorious "wogbashers." During its course, the Handsworth incident revealed the depth of interracial hostility between West Indian blacks (60 percent of whom were unemployed in Handsworth) and the Sikh immigrant group who own virtually all the area's small shops and post offices (which in England distribute unemployment benefits).

To appreciate the subversive impact of *Handsworth Songs*, one should first scan the account of the riot given in *The Times*, the "newspaper of record." Violence broke out on the warm evening of Sunday, September 9. The front page of Monday's *Times* was dominated by the headline: "HUNDREDS OF POLICE BATTLE WITH MOBS IN BIRMINGHAM RIOT." A subhead added: "Petrol Bombs Thrown, Shops Looted, Vehicles Overturned and Firemen Stoned." At this preliminary stage, *The Times* called the conflict a "spontaneous" clash between West Indian youth and uniformed authority. The spark for the outbreak was vaguely cited as a "policeman giving a black driver a parking ticket."

The mindless mob of Monday received a more refined analysis in Tuesday's *Times*. Front-page articles concentrated on stories of Asian shopkeepers being shaken down for payoffs by petrol-bomb-wielding West Indians. Prominence was given to accounts of blacks of all ages looting stores and serenely carrying off their booty in supermarket carts. The background to the riot was now cited as a police "crackdown" on West Indian "drug pushers" in the previous week.

Wednesday's front page included a vivid eyewitness account by a "Muslim community leader" who had heard "the screams for mercy of two [Sikh] brothers, beaten by black rioters in Handsworth and then left to die in the flames of their sub-post office." The triggering event of the riot was now given as the arrest of a West Indian for the stabbing of an Asian shopkeeper at noon on Sunday. All this fed *The Times*'s lead editorial, titled "MOB MURDER," which argued that the riot must have been "planned" (by West Indian "drug pushers," implicitly). Monday's "spontaneous" street violence was now Wednesday's "concerted" uprising. In the same three days, the blacks had been demonized and the Sikh shopkeepers cast as passive, law-abiding martyrs to the mob's "rampage."

The West Indians in Handsworth do not have a newspaper of record. What they do have are a filmmaking collective and Jamaican reggae. *Handsworth Songs* amalgamates video and song to contradict the official version of the riot. The hour-long film was commissioned for ITV's Channel Four, where it made a considerable impact when it was shown a year after the riots.

Handsworth Songs makes its case by montage, recurrent image, dubbed music (including a spoof of William Blake's "Jerusalem"—alluding bitterly to Enoch Powell's "green and pleasant land") and voice-over poetry. The opening sequence is dominated by shots of the starlings (other unwelcome immigrants) who infest Birmingham's central "Bull Ring." The film subsequently intercuts newsreel West Indian immigration in the 1950s with shots of the riot. Articulate Asian "community leaders" are juxtaposed with Rastafarians speaking an impenetrable dialect. And there is a witty eavesdropping on preparations for a Thames Television talk-show on the riot. The producer complains about the low light level in the audience area, and a floor manager responds "the reason

is the color of their skins." Nothing as cogent was said on the program itself.

Handsworth Songs powerfully communicates a sense of what it is to be black (more particularly, black and of Jamaican extraction) in Britain. As such, it joins a select group of films—*Babylon, For Queen and Country*, for instance—with the difference that here the mode is documentary rather than fictional. But underlying both modes is the same unjust and intractable situation. The majority of British West Indians (the term is awkward, but symptomatically so) do not find themselves in 1980s Britain by the usual immigration routes. Actively recruited by successive British governments to alleviate labor shortages in the postwar period, they came by invitation, attracted by mendacious advertisements, with the understanding that their Commonwealth status gave them equality in the mother country. Easy assimilation was assured. Two generations later, British blacks find themselves largely unemployed, wholly unassimilated, dispossessed from European prosperity, and under a government that talks incessantly about "repatriation" as an *Endlösung*. (*Handsworth Songs* intercuts an extract from Margaret Thatcher's notorious television interview in which she worries about Britain being "swamped" by black immigrants—who in fact represent about 2 percent of the population).

Handsworth Songs deals with the dilemma of the Jamaican blacks in contemporary Britain not by chronological history survey but by something analogous to memory. Seamy newsreel clips of servile immigrants of the 1950s nervously descending the gangplank at Southampton or cozily ensconced in their council house living rooms blur into the reality of 1980s inner city street life—graffiti, garbage, urban decay, and riot. The film begins and ends with a peculiarly haunting image. A uniformed black attendant, in what I take to be Birmingham's industrial museum, regards a vast Victorian flywheel—an Ozymandias-like relic of a past and wholly obsolete civilization.

Handsworth Songs is clearly a fine achievement. One must note, however, a worrying streak of dishonesty in the film. It glides briefly over the manslaughter of the two Sikh brothers (who are merely said to have "died"). It lingers at inordinate length on the deaths of two elderly West Indian women, one shot and the other harassed into a fatal heart attack by the British police. Most viewers will assume these victims died at Handsworth. They did not. They met their deaths during two separate riots in London's Brixton and Broadwater Estate.

John Sutherland

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LATIN AMERICA

Frida: Naturaleza Viva. Produced by Manuel Barbachano Ponce; directed by Paul Leduc. 1984; color; 108 minutes. Spanish with English subtitles. Distributor: New Yorker Films. Video: Connoisseur Home Video.

This film is an unusual effort to render, through a patchwork of images, a visual biography of an unusual person, Frida Kahlo, the Mexican painter (1907–1954) who was married to the muralist Diego Rivera, and whose obsessive focus on self-portraits revealed a constant struggle to position herself subjectively in the world. A surrealist who drew inspiration from Mexican popular art and song, Frida Kahlo's work challenges the viewer with her defiant gaze, her appropriation of native Tehuana, rather than bourgeois, standards of beauty, and her display of her own naked female body not as lush or seductive but as mutilated, wounded, and with heart and organs revealed (compare Jean

Franco, *Plotting Women: Gender and Representation in Mexico* [New York, 1989], 106–07). Both her work and her life lend themselves easily to feminist readings, and, indeed, Frida Kahlo has been appropriated by the feminist community.

The running titles at the beginning of the film provide the only informative text overlaying the images. The vantage point of the film, we immediately learn, will be a classic one: Frida Kahlo on her deathbed recollecting her life, in fragmented and disconnected images, as darkness falls. The titles go on to inform us that Frida Kahlo admired Karl Marx, Emiliano Zapata, and the Mexican revolution but moved away from “the iron will of Stalinism”; that the last overdue exhibition of her paintings in Mexico was a triumph, and only death, following on years of debilitating illnesses and operations that ended with the amputation of her leg, “interrupts that chaotic torrent of images.” Her work, the final title informs us, is the most highly priced of any Latin American female painter.

For the film viewer who is not already familiar with Kahlo’s life (as depicted, for instance, in Hayden Herrera’s *Frida: A Biography of Frida Kahlo* [1983]), this is not very much information to go on. But, then again, Paul Leduc’s film evokes rather than tells and makes its argument through layers of images that give the viewer the impression that one is weaving in and out of Frida Kahlo’s conscious and subconscious minds. After seeing the film, one can imagine how her waking and thinking world, her imaginative world, and her nightmare world of remembered and persistent bodily pain together formed the basis for the unique vision she brought to her canvases. It is these paintings, seen at various times through the traveling camera, that are the film’s key protagonists.

The meandering, anti-chronological structure of the film will not appeal to all historians. But the film, shot in many of the original locations, including Frida Kahlo’s residence, succeeds beautifully in giving the viewer a feeling for the historical moment in which Kahlo lived, moved, suffered, and struggled. It does so by keeping dialogue to a bare minimum and presenting scenes in which the action, not always with Frida in center stage, focuses on the popular culture of the period—the singing of popular love songs or revolutionary songs (Mexican as well as the *Internazionale*), the dancing at a New Year’s Eve party (of the tango and mambo), and the presentation of puppet shows (first by Frida’s father, then by Frida herself). The historical moment is further evoked in Day of the Dead remembrances of heroes of the Mexican revolution (which precede the movingly presented scenes of the horrible bus accident that sent a pole cutting through her pelvis and left Frida Kahlo a cripple); in the shots of Kahlo at the movies cursing when the clippings of a triumphant Hitler appear on the screen; in the various political rallies that Frida attends (in a wheelchair) in favor of Sandino and against the ouster of Guatemalan president Arbenz Guzman by the Central Intelligence Agency; and, in the interlude with Leon Trotsky, whom Frida came to love.

This situating of Frida Kahlo within history is combined with an almost eerie attempt to suspend time and view her within the confines of her own body and the pain it gave her: the film is punctuated with recurring scenes of Frida waking from nightmares in which her body, bleeding, twisted, lurching, has betrayed her. In an interview, Leduc noted that Frida was slighted in her own lifetime because, unlike the famed Mexican male muralists, “she didn’t speak of the great social movement swirling around her but rather of herself. She wasn’t the noise of the Revolution but rather its silence” (Dennis West, “An Interview with Paul Leduc,” *Cineaste*, 26, no. 4 [1988]: 55). It is the look of this silence that the film tries to recover.

During the one scene heavy with dialogue, consisting of an argument with Siqueiros, the muralist, over Rivera and Kahlo’s support of Trotsky’s exile in Mexico, Frida utters some significant lines: it is fights between political leaders (*caudillos*) that cause so many deaths and divide the people, and this male infighting is to blame for wrecking the Mexican revolution. In contrast to these words, Rivera, depicted basically as a buffoon, tells Trotsky that if he and Stalin had gotten drunk together more often, things would have turned out differently; when these lines are repeated several times, Trotsky angrily

throws to the ground a candy skull given to him by Rivera. Whatever one may think of the film's depiction of the Kahlo-Rivera-Trotsky triangle, it is to Leduc's credit that he has evoked gendered meanings in his representation of the past. And it is to his credit, too, that he did not evade the issue of Kahlo's lesbianism, depicting with both passion and humor the love that developed between Kahlo and her nurse toward the end of Kahlo's life.

While class meanings are fuzzier in the film, Leduc has also been careful not to overly romanticize Kahlo's bohemian proximity to working-class people; particularly subtle in this regard is the quick scene near the end of the film shot in a bar (*pulquería*) in which a working-class man harshly returns Kahlo's gaze (and ours), asking "What do you see?" The presence of the gaze, important in both feminist and postmodern social analysis, was a key theme for Frida Kahlo, who almost always placed herself, often as a split self, in every canvas she painted, thus rejecting objectivist, detached forms of viewing. It was the way she made herself, in her work and in her life, both viewer and viewed, self and other, that makes her fascinating to us today and has even turned her into something of a cult object. Leduc's film unquestionably evinces a touch of this cultish admiration, but its flowing and powerful collage of images also offers a new way of imagining a life history.

Ruth Behar

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Twenty Years Later. Produced and directed by Eduardo Coutinho. 1984; color; 119 minutes. Portuguese with English subtitles. Distributor: Cinema Guild.

A mesmerizing Brazilian film about the making of another film, *Twenty Years Later* is a self-consciously historical project that stretches the documentary form. The film examines what happened to the peasant actors in a radical docudrama that was aborted by Brazil's coup d'état in 1964. Rich and complex, this work raises for historians such consequential issues as the singular power of filmed oral history and the significance of self-reflexive approaches to filmed history. Documentary filmmakers often function as historians, but rarely are the processes of doing history as lucidly evident. Yet this intellectual fare is wrapped in an emotionally wrenching work that reveals the particular power of documentary film when it deals with the essentials of human existence.

Twenty Years Later is also an attempt to reclaim and interpret Brazil's recent past after years of silence. The twist is that, by using the unfinished docudrama as an entry point into the past, the entire film plays off against a document of an earlier attempt to interpret the past. In 1964, Eduardo Coutinho, a young radical Brazilian filmmaker, began to shoot an earnest docudrama of the recent murder of a peasant organizer, using real peasants as actors and with Elizabeth Teixeira, the wife of the assassinated organizer, playing herself. From the surviving footage, it is apparent that this would have been a film of noble intentions, limited significance, and minimal artfulness. Instead, midway in the shooting, the coup shut down both Brazil and production of the docudrama. Most of the peasants were jailed, the equipment and film seized, and the filmmakers fled back to the city.

In 1981, when Brazil's dictators loosened the reins, Coutinho, now well known as the scriptwriter for *Dona Flor and Her Two Husbands*, set out to make a documentary on the peasants who had acted in the film. Most impressively, he managed to track down Elizabeth Teixeira in the remote village where she had been hiding under an assumed name for sixteen years. Coutinho's interviews with her, the other peasants, and their families make *Twenty Years Later* a searing exploration of recent Brazilian history and of the costs of radical politics.

For those enamored of history, *Twenty Years Later* compels because it not only documents people recovering their pasts, it is actually part of that process. Most dramatically, Elizabeth Teixeira's true identity is revealed to her neighbors only when the film crew arrives. She then begins to reestablish contact with eight of her children whom she has not seen for sixteen years, some of whom did not even know she was alive. As Coutinho continues to return over a period of two years to talk with Elizabeth Teixeira and to search out her children, we see not only what her family has suffered in the past but also her gradual recovery of her identity, her spirit, and the remaining fragments of her family life. In a sense, we watch Heisenberg's Uncertainty Principle demonstrated on film, as the act of observing changes the observed.

As a work of filmed oral history, *Twenty Years Later* confirms the special potency of film. Over and over, we witness the power of a pause or the significance of a speaker's affect. For example, an actor-peasant who is now determinedly non-political is giving a stilted interview when the take has to be cut because of the sound of wind on the microphone. When Coutinho resumes, the man cannot or will not speak, and the eloquence of his silence and the concomitant emotion are perfectly captured on film. *Twenty Years Later* abounds with resonant moments of nuance and complexity limned by the visual dimensions of interviews.

This extraordinary work also tantalizes us with the possibilities of self-reflexive film as a historical medium. For instance, the recovered footage from the old aborted docudrama is ingeniously used to illustrate simultaneously Elizabeth Teixeira's recollection of her husband's murder and the peasants' memories of making the film. This constant eerie intrusion of the docudrama moves us to consider the intrepertive nature and the artifice of both film and history. Even more provocatively, the omnipresence of the earlier work reminds us that the filmmaker is part of this history and occupies a distinctly privileged position, one that historians may share at times.

The unfinished docudrama was a group project, a shared endeavor motivated by a common political purpose, yet the documentary investigates only the lives of the actors, the peasants. Coutinho and the rest of the film crew remain observers, even though that moment in 1964 was clearly pivotal in their own histories. We can assume that none of their lives were as devastated as some of the peasants; moreover, we know that the experiences of those peasants are now the raw material for another step in the filmmakers' careers. This is not so much a criticism as an acknowledgment of the reality of the dichotomies between the observer and the observed, between the working classes and the professional classes, and between documentary filmmaker and the filmed.

Daniel Sipe

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NORTH AMERICA

Walker. Produced by Edward R. Pressman and Angel Flores Marini; directed by Alex Cox. 1987; color; 95 minutes.
Distributor: Universal. Video: MCA Distributing.

A provocative example of postmodernism in filmmaking, Alex Cox's *Walker* is a self-conscious but entertaining exercise that raises disturbing issues about U.S. foreign relations, past and present. Based on the life of William Walker, an American adventurer who invaded Nicaragua with fifty-eight men in 1855 and became that country's president a year later, *Walker* is postmodernist in its eclectic use of narrative conventions and its

telescoping of historical events. The film draws on a variety of genres, such as documentary, docudrama, and biography, but frustrates audience expectations with respect to generic formulas; that is, the seriousness of subject matter typical of these genres is undermined (and thus reinforced) by the film's style. Similarly, the film's construction of events is chronological and linear, but the present is superimposed on the past in such a way that the historical narrative is radically informed by recent events, especially the Vietnam War.

Although *Walker* deals with political and moral issues that reverberate with the American experience in Vietnam, the mood of the film is zany, irreverent, and ironic. Cox rejects a realistic representation of historical events in favor of an emphasis on the absurd as a means of investigating the past. Toward this end, he uses a formal strategy based on a radical disjunction between image and sound in which voice-over narration, dialogue, and even background (nondiegetic) music are contradicted by the *mise-en-scène*. The effect is often hilarious. During a dinner party, Walker speaks for his deaf fiancée, Ellen Martin, who uses sign language to protest against her guest's self-righteous endorsement of Manifest Destiny and slavery. Since Walker deliberately blunts the force of Ellen's argument, viewers have to read the subtitles, "Screw your institutions!" and "Go fuck a pig!", to understand what she is really saying. A lover's quarrel ensues. Given Ellen's extreme anger, the sentimental violin and piano music heard on the soundtrack as the couple sign to each other is ludicrous. Walker swears to Ellen that he will never leave her, but he is photographed against a large model of a sailing ship.

Such examples of the primacy of the visual as opposed to audio component, or the blatant contradiction between image and sound, multiply throughout the film. Further, Cox, who co-edited as well as directed, creates juxtapositions within the frame that affect the way spectators experience time. Perhaps most amusing is the appearance of images of a twentieth-century media culture in the narrative of a nineteenth-century adventurer. While Nicaraguans flee in a horsedrawn coach, one of them reads an issue of *Newsweek* with Walker's picture on the cover, and an automobile speeds by on the dirt road. Later, as Walker explains to a reporter that the end (which he has admittedly forgotten) justifies the means, the foreground of the frame is dominated by an American drinking Coca-Cola. At the end of the film, when the capital city of Granada has been torched by Walker's army, World War II aid raid sirens signal retreat, and men who display American passports are rescued by a helicopter, that quintessential symbol of the Vietnam War. Finally, the credits roll while television reports of events in Nicaragua appear on a monitor displayed on screen right. President Ronald Reagan asserts, "Let me say to those who invoke the men of Vietnam, there is no thought of sending American combat troops to Central America." But the monitor shows U.S. fighting men engaged in maneuvers and Central Americans mourning their dead. Since television erases the past and exists only in the present, the transition from film to video as the medium for reporting events in Central America is apt. The juxtaposition of film production credits with a video monitor reminds us that all representations of reality, past and present, are ideological constructs.

Although historians may fault *Walker* for its collapsed temporality, presentism, and ironical sense of the past, the film is not constructed as an illusionist narrative but is self-reflexive; that is, it calls attention to itself as a representation, as well as to the events represented, and viewers must focus on how meaning is produced. Far from being ahistorical, the result of contemporary events intruding on the past is an awareness of change as well as continuity. As critics have observed, the postmodernist reaction to modernism, which is associated with the triumph of modernization and Western technology, has validated pluralism. In this case, *Walker* privileges women, blacks, and Third World peoples as "the other," even though Walker himself is repeatedly shot in foreground and at low angle. The film is not simplistic in its representation of "the other." A black American character remains a loyal follower even after Walker institutes slavery in Nicaragua, but another black fighter contemptuously discards his medal and rides out

of town. Nicaraguans themselves are shown as divided by political and class allegiances. Undoubtedly most amusing is heterosexual conflict, as Walker repeatedly finds himself overpowered by women with whom he cannot communicate. Ellen Martin is deaf, and Doña Yrena, the Nicaraguan aristocrat, disguises her ability to speak English. *Walker* deflates the ideological stance of its protagonist by rendering him sexually inadequate, although he claims even in defeat, "it is our destiny to be here—to control you people." Short in stature, the "Grey-Eyed Man of [Manifest] Destiny" admits to Doña Yrena he is "a shy man," but she later reassures him in Spanish, subtitled on the screen, that his performance was "not great but good enough for a gringo." Not the least of *Walker's* ironies is the parody of territorial imperative as sexual conquest.

Sumiko Higashi

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Paterson. Produced by Kevin Duggan and Geraldine Fallo; directed by Kevin Duggan. 1988; color; 37 minutes. Distributor of film and video: Kevin Duggan, Paterson Film Project, 121 Fulton St., 5th Floor, New York, N.Y. 10038.

This film is not a documentary of the dramatic silk-worker strike that rocked Paterson, New Jersey, in 1913. It is instead a fictional portrayal of downcast employees in present-day Paterson, focusing particularly on one worker who gains an enhanced sense of class consciousness by discovering the history of working-class protest in her home town. Director Kevin Duggan seems eager to suggest that dispirited workers in the 1980s can gain inspiration from labor's historic struggles and that modern workers will ultimately redeem labor's past defeats. If the silk workers lost in 1913, says one of the film's protagonists, their strike may yet be won "in history." This theme is consistent with the recent work of Steve Golin, who served as Duggan's historical adviser (see his book *The Fragile Bridge: Paterson Silk Strike, 1913* [1988]). Yet the film never adequately connects past to present, and the relationship between historic protests and current concerns remains unclear. Oddly, this ambiguity is one of the film's strongest features.

Duggan uses the relationship between two women to bridge Paterson's past and present. "Rosa" works in the old mill district, not as a textile operative but as a computer terminal operator in a high-rise office building. "Claire" is unemployed, but her parents support her as she looks for work (rarely), reads books about Paterson's industrial history (often), and strolls through the crumbling mill district musing about the plight of workers. When the two become roommates, Claire tells Rosa about their town's historic class struggle (illustrated with stills and footage from nineteenth and early twentieth-century Paterson, with oral history recordings of aging Paterson workers, and with worded messages). Claire implies that Rosa's discontent, like that of the silk workers, stems from capitalist work relations. After several disputes with her boss, Rosa apparently sees this connection and hesitantly considers organizing her fellow workers (we never learn whether she actually tries). Ultimately, the company fires Rosa, leaving her isolated and powerless (my view), the blessings of historical perspective notwithstanding.

Throughout the film, Duggan questions whether wage labor can ever be meaningful. His willingness to confront this critical issue directly and repeatedly is an important contribution. *Paterson* opens with one of Rosa's fellow workers, Amy, admitting her fear that she will never like any job. Later, when asked what her goal is, Amy wearily replies, "I want to get fired." Scoffing at Amy's lack of direction, Rosa begins to recognize her own lack of vision. Unable to imagine being in a better place, Rosa tells Claire that the very idea of going to work makes her feel nauseous and saps her energy. Most people, Rosa

says, “just turn off” in order to survive the work day and hope to “turn back on again” when they get back home. In modern Paterson, work is the absence of life. When Rosa contemplates the working career of her grandmother, who sewed forty years for the same company, she feels both admiration and pity. “Grandma” was a union activist who helped win better working conditions, but Rosa wonders whether that struggle made her daily toil meaningful, a crucial question for workers in Paterson’s post-industrial age.

Paterson’s fundamental problem is how to connect past struggles with present dilemmas. Almost everything we learn about old Paterson involves work: harsh factory conditions, artisanal pride, shop-floor struggles. But we learn almost nothing about the work of Paterson’s present-day employees—except that they hate it. Duggan never shows them on the job or explains exactly what they do. Nor is it clear that their problems stem primarily from work. A fictional labor organizer in the film states that, fundamentally, there is “no difference” between working at a loom in 1913 and typing at a computer terminal in the 1980s. Although this idea is central to the film, there is no footage here to allow us to evaluate the point.

But how *should* we evaluate history on film? If motion pictures must be judged by a different set of standards than written work, what are those standards? Heated, often confusing debates over the validity of popular historical films such as *Mississippi Burning* (1988) make it incumbent on historians to grapple with this difficult issue. There are no simple answers, but suppose we assume that filmmakers, by virtue of the ends they seek and the challenges they accept, set for themselves the standards by which their films should be judged. Take, for example, *Mississippi Burning*. However unfaithful to the historical record, the film has an unpardonable flaw: while making a statement against racism, the film itself accepts traditional racist stereotypes. This internal contradiction undermines the validity of the film’s historical “lesson.” Not plagued by racism, *Paterson* nonetheless suffers from a similar defect in its own internal logic. Being unable to bridge the past and present, the film fails to meet its self-imposed standard of validity.

Short, yet ambitious, *Paterson* tries to do too much as it attempts to connect historic struggles with contemporary problems, balance poignancy with wit, and grapple with topics as diverse as architecture and immigrant culture. It also avoids the issue of gender and work, even though the script is perfectly suited to deal with it. But Duggan’s film offers a needed break from traditional labor documentaries, with their dramatic appeals for justice and unquestioning faith in organization, and a refreshing change from Hollywood labor films, with their romantic sub-plots, predictable confrontations, and beautifully arranged sets. Creative in form, *Paterson* has an amateurish quality (choppy transitions, substandard sound, generally poor acting) that is somehow appropriate for a story of discontented, uninspired workers. Ironically, the film is interesting precisely because it makes no coherent connections between past and present, thereby facilitating debate on the changing nature of work and workers in modern America. How odd that a film should succeed by virtue of its failures.

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Radio Bikini. Produced and directed by Robert Stone. 1987; color, 56 minutes. Distributor: New Dimension Films. Video: Pacific Arts Video, 50 N. La Cienega Blvd., Los Angeles, Calif. 90211.

Robert Stone’s brilliant documentary succeeds on two levels—as a chronological treatment of the Operation Crossroads tests in 1946, when the U.S. Navy exploded the

fourth and fifth atomic bombs off Bikini atoll, and as a film about making historical film. It also explores the construction of nuclear discourse and challenges the premises of that discourse.

Radio Bikini needs to be seen in conjunction with written historical accounts, such as Paul Boyer's *By the Bomb's Early Light* (1985), for Stone does not explain many of the complicated issues. He concentrates instead on visual elements that impart a visceral understanding. We see the needle of a Geiger counter dance when a sailor's radioactive uniform is scanned, the Bikinians sail away from their ancestral homes forever, and test animals struggle to free themselves from their fate. *Radio Bikini* joins such documentaries as *Atomic Cafe* in a potent examination of the nuclear dilemma.

What gives *Radio Bikini* importance beyond its subject matter is Stone's exploration of how to use film to explore history. He relies on contemporary newsreels and military film without the use of a narrator. The navy deployed 208 motion picture cameras at the test, and the films from these are augmented by retrospective interviews with Kilo Bauno, chief of the Bikinians, and John Smitherman, an enlisted sailor. Stone's genius lies in undercutting scenes that seem normal or positive by juxtaposing them with images or sounds that carry an opposing meaning. The film opens with towering palm trees silhouetted against an iridescent orange-purple Bikini sunset, waves breaking softly on the beach. But this is no pristine paradise where "life's a beach"; it is a land of insidious, silent poison, as signaled by a haunting, elegiac melody that drowns out the soothing waves. These scenes dissolve into black-and-white newsreel footage of a delirious downtown V-J Day celebration, which Stone runs in slow motion as the Bikini elegy continues. This juxtaposition suggests that this moment of unalloyed joy is fleeting, and the use of slow motion hints at future violence and horror. President Harry S. Truman thanks God that the atomic bomb "has come to us," as if it were a divine gift and not something we had consciously sought. Stone reminds us that nuclear weapons formed a bridge from victory in World War II to the cold war's ultimate perils.

A discourse that tamed and justified nuclear weapons grew up as the sturdy twin of nuclear terror. Stone ingeniously exposes the vast resources employed to create the nuclear discourse and make it appear natural. A military officer has a translator explain to the uncomprehending Bikinians (who have not even seen a camera before) why they must give up their island for the good of humanity. We see the scene again and again as the officer does several takes for the cameras to get his gestures and inflection just right. Pompous congressmen record supposedly spontaneous statements while reading from prepared texts and trivialize the bomb by analogies to a "giant firecracker" and "atomic baseballs." The chief medical officer insists that radiation poses absolutely no danger to the servicemen. Protesters picket the Pentagon; they are literally and figuratively outside the discourse of power.

The news media eagerly further the process, uncritically relaying official statements. Human interest stories drain danger from the tests. Tongue-tied enlisted men are hustled before microphones to assure their families they are in no danger. A radio announcer reports the take-off of the plane carrying the bomb as if it were a home run for our side. The Bikinians' anguish is diminished by an announcer who describes them, without irony, as "not too wise in the ways of our civilized world."

Stone helps the powerless shape an alternative discourse that they were not able to create at the time. Bauno explains the heartsickness the Bikinians felt at their eviction—no one ate for days—as they looked back and saw the Americans burning their homes. Smitherman, a self-styled Oklahoma "country boy," laconically recounts the trust sailors invested in official assurances of safety, describes their inspection of the target ships ten hours after the blasts, and recalls being showered with radioactive spray. Stone uses the miles of navy film footage for corroboration, showing sailors ordered into radioactive areas wearing only T-shirts and thin pants, others playing shirtless on top decks. In a final scene, the camera pulls back to reveal Smitherman, who has been seen only in a tight head-and-shoulders shot, in full view. Suffering from cancer since Crossroads, he has

endured the amputation of one leg, and the other, like his hands, is grotesquely swollen. He died soon after being interviewed for the film. Stone implies that reliance on nuclear weapons, even though they have not been used in war since 1945, has already created innumerable casualties.

Radio Bikini argues eloquently that film can be used to critique the official version of events. Since that version relies today on the full panoply of cinematic techniques, film is needed to critique it thoroughly. Film may, in turn, create an alternative discourse by providing a forum for the voiceless and marginalized. Film will not displace prose for intricate argument or for conveying data; but, in probing the construction of twentieth-century discourses, a film such as *Radio Bikini* can assume a unique, essential role.

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Reviews of Books

GENERAL

PHILIPPE ARIES and GEORGES DUBY, editors. *Histoire de la vie privée*. Volume 3, *De la Renaissance aux Lumières*. Edited by ROGER CHARTIER. Paris: Seuil. 1986. Pp. 634. 350 fr.

Given the oft-noted fact that the word "privacy" cannot be adequately translated into French, it is all the more remarkable that a team made up of the most distinguished historians in France is responsible for the five-volume *Histoire de la vie privée* of which this volume is a part. The series was designed as a tribute to the late Philippe Ariès, the pioneering historian of childhood and death. Volume 3, which covers the early modern period, is prefaced by a posthumous text by Ariès himself; the questions raised in that essay are then expanded on in the introductions to the book's three sections, written by the volume's editor, Roger Chartier.

The reader who expects to find *la vie privée* neatly objectified and defined in the introductory pages will be disappointed, for Ariès and Chartier approach the notion of private life as E. P. Thompson did that of class, not as a fixed structure but as "something which happens." Casting their net as widely as possible, the volume's editors include within their definition of private life everything from the material substructure of daily life (food, housing conditions, and the like) to customs and manners in every environment from the court to the peasant household to the development of individual identity in the realms of emotion and intellect. The multiple developments affecting this broadly defined private sphere between the sixteenth and eighteenth centuries can best be explained, according to the editors, in dialectical relation to the development of effective and highly visible state power. As Chartier clearly states in the opening pages, "The gradual construction of the Modern State, not always absolutist but everywhere administrative and bureaucratic, therefore appears as a necessary condition for the definition, perception, or simple experi-

ence of a private [sphere] that was now distinct from a clearly identifiable public [sphere]" (p. 22). In other words, characteristically Western notions of privacy and individualism took shape as a corollary to the expansion of the purely public domain that the state was carving out for itself.

Inasmuch as the volume's contributors adhere to this framework (and most of them do), it is the growing rift between public and private spheres, between the state and the family, rather than capitalism or class conflict, that provides the conceptual background for essays on topics as diverse as childhood, friendship, and village ritual. It is significant that neither Karl Marx nor Max Weber appears even once in the index, while Norbert Elias is cited twelve times; the subject index gives twenty-six citations for the word *état* but only two for individualism and none at all for capitalism. It may also be of interest to note that much of the conceptual apparatus for this most recent (and ambitious) collective effort by French sociocultural historians comes from the work of German cultural theorists, including Elias, Jürgen Habermas, and the reception theorists of the Constance school. Whereas an earlier generation of historians in the Whig or Marxist traditions (Lawrence Stone or Ian Watt) interpreted the success of intimate memoirs and epistolary novels and the development of solitary reading habits as cultural reflections of economic individualism, Chartier and his contributors assign a causal status to these phenomena and give them pride of place. Three of the longest—and most interesting—essays in the volume concern forms of reading and writing, that of Chartier on literacy and reading practices, that of Madeleine Foisil on diary keeping and other forms of intimate writing, and that of Jean-Marie Goulemot on public and private genres in literature.

The editors of the volume have also sought conceptual depth in the very organization of the chapters, which eschews, for better or for worse, such obvious categories as chronology, geography, social class, stages of the life cycle. Instead, we are

offered three conceptual clusters of topics. The first section of the book focuses on the process of cultural modernization through religion and literacy (although the initial essay by Yves Castan is written in a style so dense and abstract as to leave its purpose generally impenetrable); the second section, entitled "Formes de la privatisation," deals with what one could term the transformation of personal identity and values as reflected in formal codes of etiquette, child-rearing practices, and reading and writing habits. The final chapters concern public forms of self-definition through association, ranging from friendship and family life to youth groups and voluntary associations. The complex, richly textured essays by Nicole Castan, Maurice Aymard, Daniel Fabre, and Arlette Farge in this section explore the many avatars of the triangular relationship between voluntary sociability, the rigid dictates of family life, and the growing public authority of the state. Within and against this tangle of competing loyalties, the authors suggest, modern notions of individual and familial privacy came into being.

This highly conceptual organizational scheme has the merit of avoiding banality and of highlighting novel approaches to the now-classic question of the "fall of public man." Some of the essays in part 2 expand on Chartier's insight, derived from Habermas, that one of the most important functions of the eighteenth-century Republic of Letters was to serve as a public sphere in competition with the state, founded on "the public use of reason by private individuals" (p. 23). Similarly, the last section rightly draws attention to the multiplicity of institutions that mediated between the individual and the state at a time when the antithesis of public was not so much *privé* as *particulier*.

Nonetheless, the book's topical organization does make for a good deal of repetition: for instance, Foisil's essay on journal keeping includes long sections on the writers' attitudes toward their children, which echoes the previous essay by Jacques Gélis on the individualization of children; Goulemot's chapter on genre, which comes next, includes a discussion of first-person memoirs that in turn goes over some of Foisil's ground. Such overlappings occur throughout the volume.

In the end, despite the editors' best efforts (Chartier's introductions to each of the volume's sections are brilliantly synthetic), this volume of *Histoire de la vie privée* reads less like the synthesis it is supposed to be than like a collection of often-illuminating original works of scholarship loosely organized around a common approach to the question. Many of these are well worth reading: Jean-Louis Flandrin's piece on the transformation of gastronomy from an elaborate exercise in visual

display to a more subtle form of social distinction has implications that go far beyond the *petite histoire* of cooking and eating. The editors should be congratulated on their choice of contributors and for the superb selection and presentation of the abundant illustrations. When all is said and done, however, this volume leaves one with doubts as to whether a subject as conceptually slippery as the history of private life can be coherently addressed by a team of writers. For all of its learned updating of the classics, this tome does not come close to displacing the works of Norbert Elias, Lawrence Stone, or of Ariès himself.

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EMMANUEL TODD. *The Causes of Progress: Culture, Authority, and Change*. Translated by RICHARD BOULIND. (Family, Sexuality, and Social Relations in Past Times.) New York: Basil Blackwell. 1987. Pp. xv, 217. \$34.95.

This book is a sweeping statement of a broad hypothesis on the relationship between family systems and literacy and development. It is also an attempt to provide some support for that hypothesis through a rapid survey of the West European past and the contemporary developing world. Emmanuel Todd's hypothesis is that development is a cultural rather than a material phenomenon. Beginning with a categorization of family types by degree of parental authority and women's status, Todd claims a causal link between types of family organization combining high status for women with strong parental authority and a rise in the rate of literacy. This leads to a fall in rates of mortality and fertility and, from there, to material development. It is, then, an anthropological rather than an economic model of development. It is an appealing simple argument that stands the usual approach to development on its head.

Todd's hypothesis is, however, an oversimplified view of development. Factors other than family organization and literacy are not considered. In the interests of his overall argument, Todd ignores the interactions between economic and cultural aspects of the development process. We see nothing of the ways in which children learn to read and write. Where are the funds to construct schools and train teachers found? Are we to believe that a particular kind of family organization is all that is necessary in order to have rising literacy levels? Todd's argument is further weakened by the vagueness of the variables used. Literacy, as he describes it, seems relatively straightforward: "the capacity to count and to measure," to which he adds "reading ability in

civilizations where writing is not by means of alphabetical letters—Chinese and Japanese, for example” (p. 1). This is certainly the most useful definition of literacy, and no one who has studied the question would discard such measures. But there is no consideration of the meaning of the ability to count, measure, or read. Such skills learned in a primary school of the Third French Republic surely differed in meaning from those learned by a child in Sri Lanka in the 1950s. Both Harvey Graff and Susan Hill Cochrane have shown the ambiguities of literacy in development, historically and in the contemporary developing world. But no mention is made of these ambiguities in Todd’s analysis.

This book, then, is not particularly convincing. The general argument, however, should not be forgotten, for the relationship between culture and economic development is surely more complex than either Todd or many economists and economic historians make it out to be. Todd’s insights need to be taken to more amenable surroundings, tested in regions where the evidence allows more complex models than we find here. My suspicion is that we will find that culture indeed was (and is) an important part of the development process. But we will probably not find a single rule of development as simple as the one presented in this book.

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JACK GOODY. *The Interface between the Written and the Oral*. (Studies in Literacy, Family, Culture, and the State.) New York: Cambridge University Press. 1987. Pp. xxi, 328. Cloth \$44.50, paper \$14.95.

This volume, a collection primarily of revised studies originally published during the past fifteen years, is a companion to *The Logic of Writing and the Organization of Society* (1986) and is social anthropologist Jack Goody’s most recent statement of his shifting interpretations of literate and nonliterate modes of human communications. In my judgment, it stands as Goody’s most ambitious, mature, and richest thinking on much misunderstood questions that connect closely the generally newer interests of many historians with those of anthropologists. Considered seriatim with Goody’s earlier major statements on literacy and its (at least, imputed) relationships—from the seminal 1963 “The Consequences of Literacy” (written with literary scholar Ian Watt) to the edited collection *Literacy in Traditional Societies* (1968) and Goody’s own *The Domestication of the Savage Mind* (1977) and *The Logic of Writing*—this book offers a striking if rare opportunity to watch a major scholar essaying

various efforts to contend with not only exceptionally significant but also especially slippery issues in his quest to grasp the complex interactions among modes of communication, manners of cognition, practices of expression, patterns of social and cultural organization, and questions of cultural and historical transformation. In this volume, the central “interfaces,” in complex interrelations, are those between the written and oral within societies, between cultures or societies with or without writing, and within the “linguistic life” of individuals.

Historians, regardless of specialization, may learn much from Goody—as he has tried to learn from historians and historical materials. (That is another “interface” worth considering.) Goody’s contribution is not solely a matter of his substantive findings and conclusions, some of which remain too narrow or rigidly focused or conceptualized, not well argued, and vague or fuzzy. The discrete inquiries, which do not quite make a unitary book, are exceptionally broad, nevertheless, spanning sociologies of literature and knowledge, communications, linguistics, psychology and cognitive studies, philosophy and logic and doing so historically, anthropologically, and cross-culturally. Following Goody along various paths in pursuit of “the interfaces” of the written and the oral, with their widening correlates and consequences and Goody’s self-criticisms and self-revision, can be tremendously exciting as well as instructive. These paths, as they attempt to dissect and connect the history of writing and language, uses of memory, conditions of composition and expression, forms of learning, development of knowledge, and influences on cognition, range from dichotomous and narrowly functionalistic formulations to less formulaic and sometimes much more sophisticated, subtle, and dynamic dialectical characterizations.

Goody’s writings must be read by anyone interested in the roles of communicative modes—especially but surely not only writing—in terms of organization, relationships, and changes and continuities in society, culture, and polity in their individual, collective, institutional, and epistemic dimensions. His work should also be read for his conceptual and theoretical efforts, suggestiveness of case studies and examples, and substantive issues and cases from West African, ancient Greek, and early medieval European societies and history. Some of the implications, not surprisingly, relate to contemporary debates about schooling and culture and asserted literacy crises.

Despite the brilliance of much of the work and the maturity of his revisions, Goody must be read carefully and critically. Historians have a special responsibility here. Although this new work is

marked especially by advances from reifications of "modern" Western attributes, which appear in his earlier work, and a general rejection of mono-causal approaches to and from literacy, Goody's writings sometimes abstract historical cases and developments from social and cultural contexts (for example, in treatment of written texts or ancient Western societies) and also derive from powerful psychological assumptions that are not always developed explicitly and self-critically. (His differences with Sylvia Scribner's and Michael Cole's *The Psychology of Literacy* [1982] are interesting in that respect.) Among the persistent problems that limit the success of Goody's own revisionism is his failure to fix a more complete linkage between historical dimensions and his often functional anthropology. Another problem stems from not fully maintaining the anthropological frame in some of the approaches to linguistic and cognitive transformations. To the extent that such a mutually and reciprocally reinforcing project may be attempted, Goody points in an exciting and necessary direction. To those of us centrally concerned with "the interface between the written and the oral" in the making of history, Goody, regardless of the extent to which we agree or disagree with him, firmly marks our paths and guides us, by example and as exemplar, positively and negatively, in those to follow and some of those best avoided.

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LOUIS O. MINK. *Historical Understanding*. Edited by BRIAN FAY et al. Ithaca: Cornell University Press. 1987. Pp. 294. \$29.95.

This book consists of selected essays in philosophy of history written by Louis O. Mink (1921–83), for thirty years a teacher of philosophy at Wesleyan University. The editors, Brian Fay, Eugene O. Golob, and Richard T. Vann, all from that university, have contributed an excellent introduction. Starting with Mink's unpublished doctoral dissertation, the editors first provide a summary of Mink's general views on historical understanding, in particular, that history typically affords a special "mode of comprehension" (the mode of "seeing things together," of synoptic understanding) and that historical understanding is peculiarly a matter of narrative (pp. 13, 26). The editors then use this schematism (rather than the date of composition or original date of publication) to order and to discuss the essays in this volume. The book concludes with a bibliography of Mink's writings, works mainly in philosophy of history and his

studies (mostly short notes) of James Joyce's *Finnegans Wake*. The volume is handsomely and well produced (I counted only two mistakes).

There are twelve essays total, all but two of them previously published. The first two (published in 1960 and 1970) concern the idea of differing modes of comprehension, with history and narrative fiction together constituting one such mode. The third essay is the well-known "Autonomy of Historical Understanding" (1966); its main object is to identify ways in which history differs from natural science. Essay eight, "Divergence of History and Sociology in Recent Philosophy of History" (1973), is, in my judgment, a much better account of roughly the same issues.

Three of the remaining essays are, in effect, critical studies of the contributions of other philosophers: the sixth essay (1968) is a review article concerned with books by Morton White, Arthur Danto, and W. B. Gallie; the seventh (1981) takes up W. H. Walsh's distinction of "critical" from "speculative" philosophy of history and approvingly considers attempts by Haskell Fain, Peter Munz, and Hayden White to break down that distinction; and the tenth (1978) deals with the work of A. O. Lovejoy in intellectual history. And the final two (1972, 1968) are essays on R. G. Collingwood, both of which advance the claim that Collingwood's views on history are essentially systematic and thereby require familiarity with works of Collingwood other than the *Idea of History* in order to be fully understandable.

Essay four, "On the Writing and Rewriting of History" (1972, previously unpublished), is a gem. It tells us briefly and engagingly why people (including historians) find the very idea of historical knowledge paradoxical and why, accordingly, there is such a thing as analytical or critical philosophy of history. Essay five, "Phenomenology and Historical Understanding" (no date, also previously unpublished), and essays eight (1973) and nine (1978), especially nine, all deal in one way or another with the notion of narrative and the problems it poses.

Mink attempts to persuade us of the correctness of his main view (that narrative is both essential and peculiar to the historical mode of comprehension) by grounding narrative in the historical process of change and development. Mink in effect identifies history-as-actuality with narrative structure. Or, to be precise, Mink uses two distinct senses of narrative—narrative as real structure, narrative as discourse—and argues from the first of these to the second.

This is, of course, a dubious strategy, both because it relies on equivocation (at the verbal level) and because it simply does not follow that the only or the best way to understand a thing is by

reproducing its real structure in linguistic form. Mink's position is, of course, more sophisticated than I have represented. He realizes that processes are cumulative and that retrospective understanding is necessarily different from contemporaneous understanding (even of the "same" flow of events), and only retrospective understanding is truly historical. He realizes, too, that history is not simply an "untold story" and especially not a single story. He sees that the real structures of history are not merely given. They are in fact inchoate, indeterminate; they have to be made out, just as narratives themselves must be constructed and not simply read off from events or processes. He realizes finally that retrospective understanding by any present of a radically different past (of a past whose way of life is quite different from that in the present), like the understanding of a past sharply discontinuous from the present, is deeply problematic.

Even granting him these qualifications, I still think it is unsound of Mink to argue that, because historical process is a matter of cumulative development, the mode of discourse proper to historians should be narrative in form and manner. Frankly, I find that the idea of any single general ground for historical discourse lends itself to a kind of imperialism regarding the methods of history. I also feel, looking at his work overall, that Mink's treatment of narrative discourse was, ultimately, altogether too programmatic. What we need are logical studies—as to why narratives are explanatory, as to the different sources of unity in narrative, and as to the various "tropes" or voices plausibly available to the narrativist.

Still, Mink's work (lapidary in style, though not as deep cutting as one might wish) is well worth returning to. Several of the pieces, the two on Collingwood and essays four and eight, would be especially interesting to readers of the *AHR* and would be useful either to orient the practicing historian or to introduce the undergraduate class or graduate student to the mysteries of historiography and philosophy of history.

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WILLIAM J. BOUWSMA. *John Calvin: A Sixteenth-Century Portrait*. New York: Oxford University Press. 1988. Pp. viii, 310. \$22.95.

As his introduction's title announces, in this book William J. Bouwsma sets out on "The Quest for the Historical Calvin." He seeks the John Calvin not of historiography but of history, a troubled man in his thoroughly troubled sixteenth century. Bouwsma is one of those rare hunters who bags

more than he hunts, for he returns from the sixteenth century with not one Calvin but two.

The structure of Bouwsma's "portrait," not biography, of Calvin is shaped by his discovery of two Calvins who coexisted "uncomfortably within the same historical personage" (p. 230). One was "a philosopher, a rationalist and a schoolman in the high scholastic tradition represented by Thomas Aquinas, a man of fixed principles, and a conservative" (p. 230). This Calvin tended to "static orthodoxy," abhorred "what we now call 'cognitive dissonance,'" and "craved desperately for intelligibility, order, certainty" in the face of "a terror that took shape for him in the metaphor of the abyss" (p. 230). The other Calvin was "a rhetorician and humanist, a skeptical fideist in the manner of the followers of William of Ockham, flexible to the point of opportunism, and a revolutionary in spite of himself" (p. 231). Over intelligibility and order, he "celebrated the paradoxes and mystery at the heart of existence," asserted "the primacy of experience and practice over theory," and possessed "a considerable tolerance for individual freedom" (p. 231). This Calvin chiefly dreaded what he described as "entrapment in a labyrinth" of universalized and dogmatized human thought and language (p. 231). The tensions between the two Calvins "illuminate the momentous cultural crisis central to his century, which was at the heart of the Renaissance as well as of the Reformation and as crucial for Catholic Europe as for the peoples that separated from the Roman church" (p. 4). They also "promiscuously jumbled together" elements that "have been variously combined in the whole course of Western Civilization" (p. 231).

Bouwsma's method is as unusual as the argument it presents. First, he quotes copiously from Calvin's writings without much attention to their chronological order, although he strongly favors the sermons and biblical commentaries over the better-known *Institutes*. Second, he organizes the heart of his book thematically. Following an initial portrayal of Calvin's career and anxieties and a diagnosis of his age appear three parts: Calvin's search for order and authority (the labyrinth); his search for freedom and a whole personality (the abyss); and his program for society, state, and church.

Although Bouwsma insists that both Calvins are the historical Calvin, the book's structure suggests, and its conclusion confirms, Bouwsma's belief that the humanist, rhetorical, freedom-seeking side of Calvin was the more profound, the more Christian, and the more modern and that "the composite character of his thought doomed the philosophical side of his mind, though without destroying it, to failure" (p. 231). This side Bouws-

sma tends to identify with the Middle Ages, while the rhetorical side was fated, though submerged for years in Calvinist dogmatism, to re-emerge in the riotously pluralistic, unstructured, and antiauthoritarian religious and emotional culture of modern times. This succession—modernity to medievalism, rhetoric to philosophy, freedom to authority—is Bouwsma's own resolution to the problem of the two historical Calvins, the ultimate victory of word over idea, of rhetoric over philosophy, in Calvin's own soul.

Bouwsma's book requires digestion and reflection. Cast in a beautifully clear and deceptively simple prose, studded with interesting and striking quotes from little-known corners of Calvin's works, and suffused with an intellectual tension that mirrors its subject's horror of and love for the world, this book will provoke much discussion and not a little opposition. Many readers will recognize here the culmination of the line of thought that Bouwsma began in his brilliant article, "The Two Faces of Humanism: Stoicism and Augustinianism in Renaissance Thought," published in *Itinerarium Italicum: The Profile of the Italian Renaissance in the Mirror of its European Transformations* (1975), edited by Heiko A. Oberman, and it is to Oberman's work that Bouwsma's *Calvin* stands closest in mood and vision.

Bouwsma's placement of the historical Calvin fuels at least two doubts. First, the identification of medieval philosophy with an ultimately Thomist quest for stasis, authority, harmony, and order reflects a conception of Christian Aristotelianism that scholarship (for example, that of Quentin Skinner) has largely outrun. Second, Bouwsma's feeling that the rhetorical Calvin was the more modern forces him to regard Calvinist scholasticism as a kind of relapse into "medieval" ways of thought. This notion suggests, however, a more dialectical way of relating two Calvins than Bouwsma's portrait of Calvin warrants.

This is a courageous book. Bouwsma accepts what is for those who quest historical figures a risk, a risk that the Alsatian theologian Albert Schweitzer identified eighty-five years ago. Schweitzer wrote of the New Testament scholars, "They loosed the bonds which had chained him for centuries to the rock of dogma and rejoiced, as movement and life returned to his form, and as Jesus, the man of history, moved toward them. Instead of stopping with them, however, he went right past our age and back into his own." Hammer in hand, Bouwsma sends John Calvin back into his own age. Whether forever, no one can say.

THOMAS A. BRADY, JR.
University of Oregon

MARJORIE REEVES and WARWICK GOULD. *Joachim of Fiore and the Myth of the Eternal Evangel in the Nineteenth Century*. New York: Clarendon Press of Oxford University Press. 1987. Pp. 365. \$59.00.

What do French romantic visionaries, English literary writers, *fin de siècle* figures dabbling in the occult, and a Czech poet involved in resurgent nationalism all have in common? According to this study, "all, to a greater or lesser degree, took this medieval abbot [Joachim of Fiore] as a prophet of the 'new religion of humanity' which they sought and the 'eternal Evangel' (the radical form of Joachim's doctrine) as a symbol of the 'new spirit' of the age" (dust jacket). This study by Marjorie Reeves, long distinguished for her work on Joachim of Fiore, and Warwick Gould, equally well known for his work on W. B. Yeats, not only gives us the first in-depth analysis of Joachimite influences in the modern world, but also should weaken barriers that separate the disciplines of history and literature.

The book is wide-ranging in its characters and geography. Many literary figures of the nineteenth century envisioned their age to be an era of transition to a new, last age of human history that would realize a defined perfectibility, featuring a new religion and a new religious language. As the authors state, "For some the new religion will be regenerated Christianity, for others, the new gospel will spring out of the old; for others again, it will be a religion of humanity created *de novo*. . . they looked for, or set out themselves to write, the evangel of the future" (p. 58). Reeves and Warwick are interested in the revival of the concept of the Eternal Evangel, a radical form of Joachimism that first appeared in Paris in the middle of the thirteenth century.

The problem, as always with Joachim and his followers, is trying to pin down their exact influence on later thinkers and intellectual movements. Joachim offered a kaleidoscope of images that survived below the surface of many systems and events and included some that could be adopted without accepting his theological doctrine. The literary figures presented in this study anticipated a new age of history, but, as Reeves and Gould quickly point out, Joachim of Fiore was not the first or the only one to divide history into ages. He was important, however, in establishing a dynamic relationship between historical ages. Past, present, and future, to the Calabrian abbot, were part of a continuous process with a defined goal. With such a sensual perception of the process of historical time, Joachim put the future within the grasp of the present. With a sense of expectation about the future, human beings could take the future into their own hands and not feel helpless before some

preordained grand design—thus, the reason for a renewed interest in Joachim among writers of the nineteenth century.

After introductory chapters presenting a history of Joachim and the Eternal Evangel and the religion of humanity in the early nineteenth century, Reeves and Gould proceed to the rediscovery of the Eternal Evangel by Gotthold Lessing, Friedrich Wilhelm Joseph von Schelling, Jules Michelet, and Edgar Quinet, all of whom turned back to heretics and dissidents of the medieval church for inspiration. They came into contact with Joachim and the Joachimites through different paths or from contact with each other. The bridge to the literary giants of the nineteenth century was George Sand, whose novels enjoyed widespread popularity in Europe and Russia. Her novel *Spiridion* was especially important as it was an openly Joachimist allegory.

In additional chapters Reeves and Gould follow the power of the Joachimist myth and how it caught the imagination of intellectuals in France, England, and Eastern Europe through the pens of George Eliot, Joseph-Ernest Renan, W. B. Yeats, D. H. Lawrence, and Jaroslav Vrchlický, to mention only a few.

This is a fascinating book that should furnish historians and literary scholars much fodder for debate. The style is highly technical and difficult to read in places. The frequent interlacing of English, French, and Latin throughout the text limits the study to the advanced student and the specialist. The bibliography is adequate, but it does contain some unusual omissions, for example, E. Aegerter's *L'Evangile eternel* (1928).

DELNO C. WEST
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NANCY L. ROSENBLUM. *Another Liberalism: Romanticism and the Reconstruction of Liberal Thought*. Cambridge: Harvard University Press. 1987. Pp. 225. \$25.00.

Nancy L. Rosenblum has written a book that, although not aimed at historians, should be read by them. Everyone knows that liberalism is in crisis, both practically and theoretically—John Stuart Mill's *On Liberty* was published one hundred thirty years ago—and that it needs to be reexamined both philosophically and historically in order to move forward more confidently politically. Rosenblum's subtitle indicates her focus; she seeks to give not a historical account of romanticism's relations to liberalism but a prescriptive analysis of how the two can be related to produce a more expansive liberalism. Her analysis, however, is embedded in the history of the two "isms" and is,

fortunately, articulated in terms of particular thinkers and not the vague generalities that often perplex historians of ideas.

Romanticism's first response to liberalism was antipathy: liberalism is legalistic and impersonal, it inhibits spontaneity and self-expression, and it is cold and atomistic. Discontent was expressed in what Rosenblum calls "Romantic Militarism" (chapter 1)—the glorification of war as a relief from the dull, unheroic philistine life and the law of the heart in contrast to legalism. None of this is new. But the manner in which Rosenblum raids the historical data and reconciles them with liberal thought is. So, too, is what she has to say on the issues of privacy, antipolitics (as distinct from Stoic or Christian withdrawal), heroic individualism, shifting involvements (by which she means a diversity of associations to which the individual can belong), and, lastly, communitarianism, where she does battle with a number of contemporary philosophers. On each of these issues she tries to show how liberalism can gain from romanticism and vice versa.

In the course of discussing these abstractions, Rosenblum pays attention to such diverse figures as William Wordsworth, Wilhelm von Humboldt, Benjamin Constant, Alfred de Vigny, Henry David Thoreau, the fictional Julien Sorel, and Mill. Mill is clearly her hero, embodying in his life the reconciliation of liberalism and romanticism (I cannot help citing my own book, *James and John Stuart Mill* [1975], as the actual detailing of this historical experience). One suspects, too, that Rosenblum in her own life has gone through the vicissitudes of "romantic" attempts to rethink modern society and is working through her own reconciliation. I hasten to add that such subjectivity, if I am right about it, informs but does not distort her cool, professional philosophical analysis. Where *Habits of the Heart* (1985) by Robert Bellah and others effectively exploits subjectivity in a relatively unhistorical and unphilosophical manner, Rosenblum's book moves in the other direction.

Of course, a historian might register some disquietude. Liberalism and romanticism float free of any social or political footings; although she deals with her subjects mainly in terms of their European orientation, Rosenblum's orientation is nevertheless distinctly American. Her one American thinker of note is Thoreau, but it is a Thoreau detached from reality, both his own and that of his times. And a reading, for example, of Richard Lebeaux's *Young Man Thoreau* (1975) would have been welcome. Such caveats are really beside the point, however, for Rosenblum's intention is that of the political scientist, not the historian. And she succeeds admirably in suggesting the way Western

liberalism's traditional tolerance of dissimilar values can be extended through romanticism to accommodate dissimilar sensibilities as well, without abandoning conventional liberalism's protections against political abuses. Her study is the enriching use of history, not its abuse.

BRUCE MAZLISH

Massachusetts Institute of Technology

ROBERT J. RICHARDS. *Darwin and the Emergence of Evolutionary Theories of Mind and Behavior*. (Science and Its Conceptual Foundations.) Chicago: University of Chicago Press. 1987. Pp. xvii, 700. Paper \$29.95.

Robert J. Richards's ambitious study of the development of evolutionary theories of mind and behavior fills a large gap in historical scholarship. It contains, however, a curious mixture of strengths and weaknesses. Many sections dealing with theoretical and philosophical issues require trenchant criticism, while other sections presenting a historical analysis of his main topic deserve great praise. Let me begin with the positive side of his work. Richards's primary concern is to demonstrate that the received materialistic and mechanistic view of Darwinian evolution is a distortion of the original, and he makes an important contribution to a growing body of work by revisionist scholars who have found that the initial supporters of evolutionary theory were not the antireligious determinists previously supposed. But, whereas earlier revisionists, such as Frank Turner and James Moore, reinterpret figures who can be seen as standing outside the key scientific circles, Richards has taken the next step by extending the revisionist approach to the first Darwinians themselves, including Charles Darwin and Herbert Spencer. My sole reservation in this regard is the utterly conventional portrayal of T. H. Huxley as the stereotypical materialist. Apparently Huxley is not among Richards's redeemed.

Darwin and Spencer are the focus of the first half of the book. Based on a careful analysis of published and unpublished material on Darwin's evolving theories, Richards comes to a series of intriguing conclusions: Darwin did not immediately apply the principle of natural selection to behavior; he took that step only in the 1840s after reading natural theologians such as William Kirby and Henry Lord Brougham, whose acceptance of the continuity of human and animal mind contradicted William Paley; these theorists further aided Darwin, though here negatively, by forcing him to construct a powerful theory of community selection to explain the instincts of neuter insects; and Darwin's stress on community selection in his

account of the development of human morality in *The Descent of Man* allowed him to overturn utilitarianism by moving away from selfishness as the chief motivator of human action.

Moving on to Spencer, Richards next argues that he constructed his evolutionary theory to meet the demands of his moral theory and that he should be considered the originator of evolutionary psychology and epistemology. In his excellent discussion of epistemology, Richards is particularly persuasive in illustrating how Spencer aimed to synthesize Kantianism and empiricism. The second half of the book centers on the successors of Darwin and Spencer to "test the thesis that Darwinism established a materialist, mechanistic, and amoral hegemony in late Victorian science" (p. 332). Richards succeeds in undermining that thesis in his examination of George Romanes's faith in an evolutionary universe governed by divine purpose, the metaphysical monism of Conwy Lloyd Morgan, William James's use of Darwinian theory to posit mind as an active force in the evolutionary process, and James Mark Baldwin's belief that natural selection worked to maintain socially cohesive groups made up of other-regarding individuals. The final chapter explains how the humane, religious, and moral quality of nineteenth-century Darwinism has been obscured through its transformation by twentieth-century developments.

Unfortunately, Richards believes that all of his insights into the Darwinian tradition are derived from his position on ethics and his adoption of a particular historiographical model, both of which are touched on throughout the book and laid out systematically in two appendixes. I enthusiastically endorse the thrust of Richards's revisionist history of evolutionary thought, yet, when he speaks as a theoretician and a philosopher, I find myself in total disagreement. Richards wants not only to show that Darwinian ethical theory was no crude utilitarian morality of selfishness but also to defend the philosophical validity of evolutionary ethics. He even praises Spencer's ethical ideas as "morally admirable" (p. 303). Richards seems bent on replaying those late nineteenth-century debates over evolutionary ethics, and he is willing to enter the fray on the side of Huxley's opponents. This is not the time or place for an extensive philosophical refutation of evolutionary ethics, but readers should judge for themselves whether Richards supports his claim that evolutionary ethics is no more fallacious than other moral systems in its derivation of "ought" from "is" (p. 620).

Equally disturbing is Richards's defense of what he calls the natural selection model for history of science. Richards justly asserts that adoption of a model is necessary for historical work. He is also to

be commended for explicitly articulating his own model as well as analyzing in his first appendix other models used in the historiography of science. But his model is problematic, particularly in light of the subject matter to which it is applied. Richards believes that scientific conceptual systems are comparable to organisms or biological species that compete for survival with rivals within a common environment. In the case of the history of science, the environment is composed of complex intellectual, cultural, social, and psychological factors that exert pressure on developing conceptual systems; the rivals are other conceptual systems, the struggle keenest among those that are most similar; and the survivors are those systems of scientific ideas that best adapt to the different conceptual pressures of the environment while meeting the challenge of their competitors.

Does it make sense to adopt a Darwinian historiographical approach, no matter how sophisticated, to study Darwinism? Will we be provided with a position outside Darwinism from which to examine it? Richards argues that his natural selection model (NSM) is superior because he can draw on the virtues of all other models. Yet he rarely strays from a consideration of the intellectual context of scientific ideas. Apparently his notion of social context is limited to an analysis of psychological factors as in his study of James or a discussion of the politics of the scientific community as presented in the chapter on Baldwin. Furthermore, Richards's NSM does not escape the charge of reductionism that he levels at the sociobiologists. Conceiving of the development of humanly created scientific ideas as if analogous to the evolution of species in nature is reductive at its very roots and in flagrant contradiction with Richards's attempt to rescue nineteenth-century Darwinian thought from its putative materialism.

In an interesting section on the politics of scientific ideas, Richards discusses how Baldwin's knowledge of the structure of scientific communities enabled him to foster the survival of his ideas, a sure sign of the fitness of his work (p. 490). After reading Richards's book, I suspect that he perceives not only the ideas of scientists to be subject to the process of natural selection but also the ideas of historians of science. Richards's relish for the novelty of his own views, his aggressive attacks on competing historiographical models, and his attempt to overwhelm the reader with information (the book runs well over six hundred pages) all seem to make sense in this light. So does his doubtful claim that in formalizing the natural selection model he has articulated explicitly what a new generation of Darwin scholars have implicitly assumed (p. 558). Perhaps Richards is only imitating the successful evolutionary strategy of Baldwin,

who "knew that controlling the terms under which ideas traveled and making others use your language identified the ideas with you" (p. 490). But what will likely survive is Richards's revisionist interpretation of nineteenth-century theories of mind and behavior and not his attempt to build an evolutionary history of science.

BERNARD LIGHTMAN
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PETER GALISON. *How Experiments End*. Chicago: University of Chicago Press. 1987. Pp. xii, 330. \$14.95.

In choosing for his title the phrase "how experiments end," Peter Galison has put initial, primary emphasis on the criteria that experimentalists adopt for believing their evidence (observations) to be conclusive. But this consideration is only part of the penetrating analysis included in this volume. As Galison points out, most histories of science stress theoretical, conceptual development. But the relation between theory and experiment is of profound importance for a genuine understanding of the scientific endeavor—all the more so in recent decades.

Galison has set himself two goals: analyzing modern experiments in microphysics with sufficient detail to capture the debates and assumptions that lead experimentalists to accept an effect as valid and charting the course of twentieth-century changes on the road to experimental commitment. The fact that the examples chosen by Galison to illustrate his perspective are all from twentieth-century physics is not surprising in light of his own background and interests. He is the holder of two Ph.D.s from Harvard University—one in elementary particle physics, one in the history of science.

The history of twentieth-century physics is a fascinating and complex field. In the decades since 1900, research in experimental physics has changed from relatively isolated investigations performed in laboratories of individuals to large team efforts involving huge, expensive equipment with computerized monitoring and recording devices. Furthermore, researchers have moved from the realm of macrophysics to that of microphysics in terms of events being investigated. These changes have affected the way experimentalists approach their equipment and interact with their fellow experimenters and with their theoretical counterparts.

Another relevant development of recent decades is the extent to which physics has become interesting to historians, philosophers, and sociologists of science. Galison is well acquainted with

the works of such scholars and sets his own presentation in the context of contemporary analytical thought.

He has chosen to illustrate the methods, outlooks, and behavior of experimental physicists of recent decades by focusing on just a few examples, happily so in my opinion. The examples considered are the relation of magnetism and rotation, the discovery of the muon, and the demonstration of the existence of neutral currents. All three topics have intrinsic interest, and they were investigated by more than one set of experimenters, each approaching the task from a different perspective. This method of presentation not only clarifies the significance of the results but also demonstrates that progress in physics does not proceed along a smooth, straight course. By choosing such a limited number of examples, Galison is able to devote enough attention to each to provide a more complete history of the episodes described than is presently available anywhere else.

The readers who will be most intrigued by this approach and most informed and enlightened by this presentation are persons with backgrounds and interests most closely paralleling Galison's. It would be regrettable, however, if others did not delve into the contents of this book, even where the details of the physics involved are not always comprehensible to them. Galison provides good introductory material, accessible to any interested, educated person, and has two summary chapters to reiterate, clarify, and expand his points.

This is a carefully written and well-researched piece of scholarly investigation, worthy of careful attention and effort on the part of the reader who will be rewarded by attaining a higher level of understanding of contemporary physics in particular and science in general.

KATHERINE R. SOPKA

Four Corners Analytic Sciences

RUSSELL C. MAULITZ. *Morbid Appearances: The Anatomy of Pathology in the Early Nineteenth Century*. (Cambridge History of Medicine.) New York: Cambridge University Press. 1987. Pp. ix, 277. \$47.50.

This clever book's clever title does not fully prepare the reader for what is to come. Russell C. Maulitz has written a comparative *Rezeptionsgeschichte* of Xavier Bichat's tissue-based pathological anatomy in France and England. In his native land, Bichat was still a relative outsider to the medical establishment at the time of his premature death in 1802, but within a few decades he had become what Maulitz calls a "talismanic" figure

(p. 5), and his approach occupied the very core of French medicine. Across the Channel it was a different story. The medical world learned of the work of Bichat and his successors through translations, journal reports, and the experience of a generation of students who went to Paris following the Napoleonic wars, but French-style pathological anatomy was not easily established on English soil.

Maulitz attributes the success of pathological anatomy in France not merely to its intrinsic scientific merits but also to a peculiar conjunction of intellectual, professional, and institutional factors. Its introduction roughly coincided with the union of medicine and surgery, formally joined by the revolutionary reorganization of medical education in 1794. In the Old Regime, Maulitz argues, physicians and surgeons were characterized by essentially different mentalities, the one holistic and primarily humoralist with a theoretical bent, the other localist and primarily solidist with a clinical orientation. Pathological anatomy provided a common language. It associated disease with local lesions, but the tissues in which they occurred were widely distributed and allowed the disease to manifest itself more generally in the body. The tissues belonged to solid organs, but their membranes produced fluids whose quantity and quality were altered by disease. Theory was linked not only to empirical observations at the dissection table but also to physical diagnosis (which could later be correlated with post mortem findings) and intervention (such as paracentesis to drain excessive fluid). These concerns came together in the work of Bichat's greatest continuator, René-Théophile-Hyacinthe Laennec. Maulitz sees Laennec's *Traité de l'auscultation médiate* (1819), with its celebrated discussion of the stethoscope, as primarily a work on pathological anatomy.

In England, Maulitz suggests, the tripartite division of physicians, surgeons, and apothecaries held firm and militated against the new synthesis. Pathological anatomy found adherents on the fringes and outside of the medical establishment in Scotland (especially Edinburgh), at the new University of London (1828), among Dissenters and reformers such as Thomas Hodgkin, and among the new breed of surgeon-apothecaries who worked as general practitioners and challenged the claims of the old privileged bodies. But the establishment resisted, and some physicians sought to create a distinctly English pathological anatomy, closer in spirit to John Hunter than to Bichat.

Although some of his themes will be familiar to specialists, Maulitz's closely argued book breaks new ground in a well-worked field; it is a measure

of his originality that he can, refreshingly and quite appropriately, confine Michel Foucault to a single endnote. To be sure, like many authors with a good argument, Maulitz may in some ways overstate his case. He perhaps exaggerates the gulf between physicians and surgeons in nineteenth-century England, and some contextual factors, such as resistance to dissection in English society or the role of the French state as sponsor of science and medicine, may deserve more emphasis than they receive. One wonders, too, whether English complaints about French theory mongering and the "Gallomania" of misguided compatriots may not stem from broad cultural biases. But Maulitz makes an important point. Medicine was not a unitary discipline in the Western world; national differences mattered. It is to be hoped that his study will inspire future work along similar lines.

MATTHEW RAMSEY
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STANLEY W. JACKSON. *Melancholia and Depression: From Hippocratic Times to Modern Times*. Yale University Press. 1986. Pp. x, 441.

The latest in a long line of anatomists of melancholy, Stanley W. Jackson provides brief and lucid discussions of leading medical writers' ideas about the symptoms and treatment of the malady. He traces the subject from its beginnings in the Western tradition in ancient Greek medicine to its present manifestations in contemporary psychiatric, psychoanalytic, and psychobiological literature. In one respect, Jackson's book resembles the wonderful compilation of his greatest predecessor, Robert Burton. Like Burton's *The Anatomy of Melancholy*, this volume is a book about books, a distillation of earlier authorities rather than a study of men and women who have suffered from melancholy and depression. Jackson lacks Burton's incantatory prose style and his rambling, impulsive imagination, but he possesses the more mundane virtues of clarity and fidelity to the texts he discusses. It is these qualities that make his book the most useful introduction to the history of ideas about melancholy in English.

Jackson divides his survey into three main parts. The longest section surveys medical writers' descriptions of melancholy and their therapeutic suggestions over the last three thousand years. The main theme of these chapters, each of which is devoted to an era or century, is, as Jackson puts it, "variations in a remarkable consistency." He argues that, despite differences in the manifestations of the signs of melancholy or depression as one culture succeeded another, the essential as-

pects of the malady have remained the same throughout Western history. Fear and sadness, or anxiety and depression as we might call them, have been its chief symptoms over the centuries. For a very long time, perhaps until the nineteenth century, grand delusions were also associated with it. This basic core of symptoms was elaborated and explained by the medical writers of different periods in terms that reflected the science, theology, and psychology of their times.

The second and third parts of the book are more miscellaneous. They contain a set of chapters discussing the relationship between melancholy and other afflictions—notably mania, hypochondria, grief, and diabolical possession. The book closes with chapters on three decidedly exotic illnesses, lycanthropy, love-melancholy, and nostalgia, that are closely related to melancholy. These discussions veer back and forth in temporal perspective. The analysis of mania and melancholy, for example, is rooted in a contemporary debate about the validity of the concept of manic-depressive psychosis. The discussion of lycanthropy, on the other hand, is inevitably much more antiquarian. If werewolves may still be found in the consulting rooms of contemporary psychiatrists, they are surely no longer diagnosed as melancholics.

Jackson's discussions of medical ideas and treatments are invariably reliable and sometimes entertaining. Perhaps that is all it is reasonable to ask. One wishes, however, that the author had been bolder in his attempts to explain why some symptoms of depression have remained stable in descriptions of the malady and others have changed. His explanatory scheme presupposes that there is some underlying factor or factors that account for the perdurable continuity of the core symptoms and that superfluous complaints, attached to the core by the force of culture, have been gradually shorn away by increasingly accurate clinical observation. And yet Jackson will not share with us his opinion about what the underlying factors responsible for the "remarkable consistency" he has discovered might be. All of the possibilities are reviewed—or at least touched on—but none is endorsed or even discussed in relation to the book's thesis. This approach may be regarded from one perspective as admirably cautious, given the fissures in contemporary psychiatry, but it is also frustrating. For in the end the most difficult methodological problems posed by the history of psychiatry have been, though noticed, evaded. In spite of its many virtues, therefore, this book ultimately fails to advance very much our understanding of the meaning of melancholia in Western culture or to suggest fresh perspectives from which it may be studied. Historians concerned

about such issues will have to rely instead on the work of psychiatric anthropologists, notably Arthur Kleinman, whose *Social Origins of Distress and Disease* (1986) should be read in conjunction with Jackson's descriptive history.

MICHAEL MACDONALD
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DANIEL R. HEADRICK. *The Tentacles of Progress: Technology Transfer in the Age of Imperialism, 1850-1940*. New York: Oxford University Press. 1988. Pp. x, 405. Cloth \$32.50, paper \$11.95.

In *The Tools of Empire* published in 1981, Daniel R. Headrick sought to demonstrate that technological advance was the major determinant of European imperialism. He accordingly traced the development of innovations such as the rapid-firing rifle, quinine medication, the steamship, the submarine cable, the railroad, and canal-dredging machinery and established their importance in facilitating European domination of most of the world.

Now, in this sequel, Headrick asks why formerly colonized regions of the world failed to industrialize despite the massive technology transfers of the imperial era. He seeks to demonstrate that it was the unequal power relationship between the colonizers and their colonial subjects that thwarted indigenous attempts to industrialize. To support this thesis Headrick once again traces the development of many of the technological innovations described in *Tools of Empire* and adds new sections on urban public health, botanical research and plant transfer, and mining and metallurgy.

For example, the author demonstrates that railroads were built "to make India useful to Britain" (p. 91). British authorities discouraged the ancillary industries that usually accompany railroad development in order to protect their domestic industries. In contrast, independent Japan developed railroads for its own benefit. In the chapter on mining, we encounter indigenous enterprise for the first time in the persons of Jamsetji Nasarwanji Tata and Pramatha Nath Bose, founders of the Indian steel industry. Once again, however, imperial interests caused delays and setbacks in India's industrial development despite its promising start. After surveying technical education in colonies, the author concludes that colonial rulers "educated their subjects up to a point. Beyond that point they withheld the culture of technology" (p. 345). In a chapter entitled "Experts and Enterprises," African and Asian entrepreneurs similarly found themselves limited by the commercial and political interests of the metropolises. In a brief concluding chapter the author states his thesis that

technology transfer "came wrapped in flags" (p. 379). He suggests that, if Europeans had not conquered them, Asian and African societies might have retained their traditional ways for a few more generations or might have become both modern and industrial. Instead, the imperial powers and private business interests developed the colonies for their own benefit, and, thus, the colonies became "both modern and underdeveloped" (p. 384).

I felt a distinct sense of déjà vu in reading the chapters on technology transfer because the author has merely transplanted much of the information found in *Tools of Empire* to *Tentacles of Progress*. He does not cite recent ground-breaking research on education, irrigation development, and efforts at industrialization in Egypt that might have modified his account. In general, the cultural orientations, historical contexts, and internal dynamics of the indigenous societies under discussion do not spring to life. There is no analysis of technology transfer and gender, although there is a substantial literature on the subject.

Headrick's thesis has been advanced many times in the past, but few scholars have brought together such wide-ranging and diverse materials to support it. In consequence, it is a book worth reading.

NANCY E. GALLAGHER
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COLIN G. CALLOWAY. *Crown and Calumet: British-Indian Relations, 1783-1815*. Norman: University of Oklahoma Press. 1987. Pp. xiv, 345. \$21.95.

British-Indian relations in the three decades after the American revolution were based on self-interest as much or more than in the colonial era. The home government cocked an experienced and greedy eye on the fur trade and uses of Indians in the trade. To protect this valuable resource, the British government wanted friendly alliances with the tribes. As the Indians well knew and freely stated in treaty talks with the British, these policies also benefited them. Keeping a lucrative alliance in the rich fur trade and fending off American land speculators and settlers helped the Indians to preserve, for the time being, much of their way of life and landed heritage.

Colin G. Calloway's excellent book, based on a large mass of manuscript and printed materials in the United States, Canada, and England, fills a gap in Indian history by explaining and analyzing British-Indian relations in the postrevolutionary era. Traditionally, this period is portrayed as a contest between Britain and America for control of the great inland frontier centering on northern

fortifications occupied by the British. Calloway's book shows that the story was much more complex. He is interested not so much in providing a narrative as in seeking to understand how the British and Indians dealt with each other. British conceptions of Indians, Calloway shows, were highly ethnocentric, based on ideas of cultural superiority. The British nonetheless were bound by practicalities in judging Indians on their performance as allies in fur trading and in war. Tribesmen had their own pragmatic motives for engaging in trade and in agreeing to alliances, particularly if they could negotiate expensive gifts at treaties.

Calloway's study is thus about Indians and the British as partners. It is divided into four parts: the first is a survey of the British and Indians in historical contact, especially in the northern Indian Department. Part 2 plunges into the actual meeting of cultures, evolving British and Indian stereotypes, and turn-of-the-century ideas of "savagery and civilization." Part 3 takes up the mechanics of the fur trade, Indian and British traders, and middlemen. Part 4 analyzes the "uneasy alliance" between Indians and the British, based on mutual needs. The book culminates in a discussion of the treaty of Ghent, which led to a gradual withdrawal of the British from Indian affairs below the Canadian border. Native people here were eventually left to the mercy of land-hungry Americans.

Here is ethnohistory at its best by a seasoned scholar in the field. Calloway knows his subject and writes convincingly. Environmental historians, however, will lament that the author has nothing to say about violence inflicted on fur-bearing animals and the near decimation of certain kinds of wildlife by the fur-trading complex. As Frederick Merk demonstrated in editing Hudson's Bay Company records, the company knowingly encouraged, even forced, Indian people (who were dependent on company trade for guns, hatchets, and strouds) to embark on campaigns exhausting fur-bearing animal populations when profit was needed to balance the books. However ecologically destructive it came to be (as David J. Wishart has demonstrated), the fur trade was a vast commercial enterprise involving generations of Indians and whites who appeared to believe that the animal supply was endless. But this is perhaps another book, yet to be written. Fur-trade historians will conclude that Calloway has given us a penetrating inside account of the British and Indians in the era following the revolution.

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CARL E. SOLBERG. *The Prairie and the Pampas: Agrarian Policy in Canada and Argentina, 1880-1930*. (Comparative Studies in History, Institutions, and Public Policy.) Stanford: Stanford University Press. 1987. Pp. xii, 297. \$39.50.

Like the weather, everybody talks about comparative history, but nobody does anything about it. The late Carl E. Solberg, whose death at forty-five tragically ended a productive career, was a salutary exception. This, his last book, is a model of closely reasoned analysis, and it demonstrates the way explicit and careful comparison refines old questions and raises new ones. As Marc Bloch pointed out sixty years ago, the trick in comparative study is to find things to compare that are similar. Solberg's units of analysis are nation states, and Canada and Argentina between 1880 and 1930 provide, or, rather, Solberg makes them provide, excellent units for comparison. In both countries enormous areas of virgin grasslands—the prairies and the pampas—were broken to the plow as rail and steam provided opportunities for cereal export to European markets. Both countries had vast natural resources, the timing and volume of exports were similar, and in both cases the indigenous people and their descendants, the *métis* and *gauchos*, were either destroyed, absorbed, or replaced by immigrants from Europe. But all of these similarities produced strikingly different results. Canada after 1930 retained its export markets and moved on to industrialization, development, and democracy, while Argentina's promising "takeoff" into growth crashed into the rubble of lost markets and political chaos.

How can such different trajectories be explained? Dependency theories and world-systems theory predict a melancholy fate for both; a facile historical account might emphasize the difference between Northern European and Mediterranean cultural antecedents. But Solberg seeks explanation in political economy and, more particularly, in the role of the state and its interaction with class interests in shaping agrarian policy in the two countries. Perhaps the clearest example of this interaction is the "National Policy" carried out by Sir John Macdonald's Conservative party, which represented in Ottawa the merchants and nascent industrialists during the last third of the nineteenth century. Their class interests led to the formulation of tariff policy to protect industry, the promotion of railroads, and an insistence that immigrants be settled on their own land in a Canadian version of the Homestead Act where they were able to produce staples for export and become consumers of Canadian industry. In Argentina, on the other hand, the landed elites, especially the great cattle barons whose origins can

be traced to the late eighteenth century, were all powerful, and their class interests led them unhesitatingly into an alliance with Great Britain in order to obtain a market for exports, sources of credit, and luxury imports. From these differences flow secondary but illuminating elements in the comparative argument. Most immigrants to Canada accepted citizenship (a requirement for a homestead grant), organized themselves into rural cooperatives, and gave rise to a mass democratic electorate in the prairies that brought political pressure on Ottawa. In Argentina, Spanish and Italian workers at first were attracted by Southern Hemisphere employment and, like opera singers, worked there in the off-season and returned to Europe with their wages. Those who stayed were very slowly assimilated into Argentine politics by an elite that feared the consequences of a wider franchise: in 1914 only 1.4 percent of the foreign-born were naturalized. Their politics took the form not of votes but of violence and strikes, and, paradoxically, the great Argentine landowners were quicker to mechanize wheat farming in order to avoid labor problems than were the Canadian family farmers. It is also true that, whereas Canadian farmers pushed for and got reform, the powerless Argentine tenant farmers received the lash of repression and stunted political development. These are only a few of the elements that come to light in this fascinating, nuanced, and qualified study that I hope, in such a short review, I have not made to sound overly deterministic.

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ANTHONY CAREW. *Labour under the Marshall Plan: The Politics of Productivity and the Marketing of Management Science*. Detroit: Wayne State University Press. 1987. Pp. 293.

This book, whose scope is somewhat broader than indicated in the title, challenges some of the certainties of the Marshall Plan's most outspoken detractors as well as those of its most avid champions. Anthony Carew reveals that the European Recovery Program (ERP), which administered the famous aid scheme, neither split the European labor movement nor introduced a new gospel of productivity that revolutionized it. The influence of the Marshall Plan was both more obvious and more subtle.

The European labor movement was badly divided long before the Marshall Plan announcement, according to Carew, with "holes [in the post-1945 common front] beginning to appear . . . well before the full weight of American policy

began to have an impact on domestic European politics" (p. 225). The most important rupture was between Communist and non-Communist unions, but the persistence of nationalist outlooks also prevented many old scars from healing. Detecting a general "lack of common purpose and organizational cohesion on the part of European labor," Carew concludes that proposals for a "socialist Third Force" standing between the two blocs were little more than pipe dreams and that the absence of a sharp international focus made labor the captive of conservative governments and American-made productivist ideologies.

Disputing the idea that labor played, or was ever intended to play, a prominent role in the Marshall Plan, Carew traces the genesis of labor's role back to progressive businessmen associated with the Committee for Economic Development. For them, growth and not labor partnership was the key to stability. In their perceptions, unions were only an afterthought with no clear-cut purpose other than boosting labor morale. The author also points out that American unions seldom shared management responsibility after World War II; rather, they forsook this opportunity for guarantees of high wages. Macrolevel industrial planning by labor did not exist except in the minds of trade union intellectuals. They, however, were the ones who, as labor attachés under the Economic Recovery Program, had the task of instilling in European union officials American ideals of modern labor relations.

Their success, or lack of it, is the concern of Carew's final chapters. His general conclusion is that large amounts of money, and little else, was what brought results. European labor officials welcomed "study tours," all expenses paid, to the United States, not to mention plans for American-funded research institutes in their own countries. The author does not deny that over the long run this kind of investment had an impact, if only in familiarizing Europe's non-Communist union leaders with the jargon of American management science. Its immediate influence was less evident, however. European labor officials by no means took the doctrine of productivity to heart. The British Trade Union Council looked upon the work of the Anglo-American Council on Productivity, which was supposed to provide a showcase of American-European cooperation, as partly irrelevant, partly inhumane, and worthy of interest only in providing the union movement with a possible lever of power. The ERP-sponsored National Productivity Centers set up in France, Italy, and West Germany encountered similar receptions. Carew shows that even American labor became disenchanted with the Marshall Plan. Although some of the difficulty arose from intramu-

ral feuding between the AFL and the CIO, the real source of the problem was the growing awareness that union representatives were unable to influence policy or in fact to do more than make propaganda on behalf of the ERP. By mid-1950, according to Carew, "cynicism" pervaded the Labor Division of the ERP's European branch, the Economic Cooperation Administration (ECA). This disenchantment did not, however, cause the ERP to grant real authority to the Labor Division. Instead, Washington increased its already bloated and largely idle staffs, expanded its budget, and shifted its mission from promoting productivity to dealing directly with the Communist threat.

Carew's polished, temperate, persuasive, useful, and highly original account describes one important instance of the limits on American power to reform or otherwise influence events in Europe after World War II. As such it makes a substantial contribution to the emerging debate between those who emphasize the primacy of the American role in shaping the new Atlantic order and those who claim that its importance has been exaggerated. Carew's book can also serve as a warning against placing excessive interpretive weight on the existence of "corporatist solutions," which, some historians maintain, brought European labor into full partnership with business and the state in the decade after World War II. Finally, Carew casts doubt on the "socialist alternative" to Europe's post-World War II development. The supposed course not taken may never have been there in the first place.

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TIMOTHY J. BOTTI. *The Long Wait: The Forging of the Anglo-American Nuclear Alliance, 1945-1958*. (Contributions in Military Studies, number 64.) New York: Greenwood. 1987. Pp. viii, 274. \$39.95.

Generally speaking, Anglo-American relations during the cold war were very close. This closeness was the result of a common heritage, the grand alliance during World War II, and what was perceived in Washington and London by 1946 as aggressive Soviet behavior. Timothy J. Botti has demonstrated that, although the two great democracies were forming closer policies on almost every front, this "degree of intimacy did not develop, however, in the critical field of atomic energy" (p. 2.)

Botti's thesis is straightforward. Great Britain had assisted in developing the atomic bomb and at the end of the war desired information, a small supply of bombs, and cooperation on commercial

development of atomic energy. Yet U.S. officials would not consider British requests, for the Truman administration considered the atomic bomb America's sacred trust, and Congress agreed by passing the Atomic Energy, or McMahon, Act. The Klaus Fuchs spy scandal also hurt the British case, for Americans felt that security standards in Britain were too weak to protect American secrets and that such leaks might have contributed to the first Soviet bomb in 1949. Furthermore, American policy makers wondered what the United States had to gain from more cooperation; the British nuclear program was slow and did not produce Britain's first bomb until 1952. Winston Churchill realized the problems, and, when he returned to the prime ministership in 1951, he attempted to restore America's confidence that Britain could be a reliable nuclear partner. He tightened security, increased coordination between British and American chiefs of staff, stockpiled atomic weapons, and undertook development of the hydrogen bomb. Shortly thereafter, Dwight Eisenhower became president, and Russia detonated a hydrogen bomb, two factors that resulted in a renewed American desire to increase cooperation with London. The two nations moved closer together and began to share commercial and military nuclear information, and after the Suez crisis the Eisenhower administration offered London intermediate-range ballistic missiles with atomic warheads and nuclear submarine propulsion information. A long-term alliance, however, was not signed until the West was provoked, again, by Soviet action, namely, Sputnik. As Botti concludes, "Frightened by the Soviet Sputnik successes, the Eisenhower administration accomplished more in Anglo-American nuclear relations in two months than American officials had in twelve previous years" (p. 210). In July 1958 the two democracies signed the Anglo-American bilateral agreement.

Botti has detailed an important aspect of Anglo-American cold war relations. He has incorporated some documents retrieved by use of the Freedom of Information Act. His chapter on the impact of Sputnik is interesting, and his introductory and concluding chapters outline the main themes of nuclear relations between Britain and the United States.

Yet there are many problems. No British documents have been used. Furthermore, other sources such as popular opinion polls and editorials have not been consulted, so one gains little understanding of British and American domestic problems and policies that influenced foreign affairs. The result is a book that takes the reader from one meeting to another in a seemingly endless conference. Finally, historians will wonder why a monograph published in the 1980s concern-

ing the United States, the bomb, and nuclear policy during the cold war never mentions atomic diplomacy, the New Left, or postrevisionists.

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BRUCE W. JENTLESON. *Pipeline Politics: The Complex Political Economy of East-West Energy Trade*. (Cornell Studies in Political Economy.) Ithaca: Cornell University Press. 1986. Pp. 263. \$29.95.

In his preface to this volume, political scientist Bruce W. Jentleson places his work within the context of the reawakened field of political economy, a melding of politics and economics that has brought this nineteenth-century term back into scholarly usage. As a case study of East-West energy trade from 1945 to the present, the book also contributes to recent American history as it unravels the factors that have shaped the foreign economic policy of the United States toward both its Western allies and the Soviet bloc. Historians, too, have shown interest in the political economy approach, but rarely do they focus their work so overtly on policy. This is not meant as criticism but as emphasis on the very different approach of the political scientist from that of the historian.

Jentleson traces East-West energy trade during four chronological periods: the cold war era when U.S. power and prestige made Western trade sanctions against the Soviets effective; the period up to and including the controversy over the Friendship Oil Pipeline in 1962–63 that exhibited cracks in the Western alliance; the emergence of détente under Richard Nixon and Henry Kissinger when the carrot of increased trade was put forth as an alternative to the stick of sanctions against Soviet behavior; and the Reagan administration's flawed attempt in the early 1980s to hinder construction of the Siberian natural gas pipeline through Western trade sanctions as a protest against Soviet policy in Poland—the major policy issue at which the book is directed. The historical chapters exist primarily to provide background on the Siberian pipeline issue, to demonstrate that the strains in the Western alliance that emerged over this controversial attempt to coerce the Europeans to follow the American lead in cancelling the export of gas turbines for pipeline compressor stations had antecedents. The historical discussion is organized well, clearly presented, and based on research in relevant archival sources, published secondary studies and memoirs, as well as interviews with relevant actors.

The major thrust of the book, however, is policy analysis and not historical inquiry in its broader sense. The author employs the case study of

East-West energy trade and pipeline sanctions to illustrate his concluding argument that a strategy of "economic inducement" (through cooperative trade) rather than one of "economic coercive power" represents a more rational approach to influencing Soviet behavior in today's complex world (p. 246). The historical analysis demonstrates that the relative weakening of the American economy and strengthening of the Western European economies has had a direct influence on constraining the coercive options of the United States. Thus, the most useful contribution of this book from the perspective of the *AHR* reader is its demonstration of the value of history to applied policy analysis.

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ANCIENT

SIMON HORBLOWER. *Thucydides*. Baltimore, Md.: Johns Hopkins University Press. 1987. Pp. ix, 230. \$25.00.

This is an intelligent, useful book that is clearly and crisply written. Its chief virtues are an excellent grasp of the modern scholarship on the subject and a bracing infusion of common sense into the debate. Scholars of the nineteenth century tended to view Thucydides either as a dispassionate model of Rankean scientific historical objectivity or as the precursor of a rigorous scientific sociology. In the last quarter-century the opposite tendency has increasingly held sway, that is, to emphasize his subjectivity and artistic, not scientific, characteristics. This view has sometimes gone so far as to obscure the fact that the historian of the Peloponnesian War put very special emphasis on his desire to tell the story and its details as accurately as possible. Unlike the poets who could change and manipulate their myths and were expected to do so, he was bound by the facts and the sequence of events, as historians ever since have thought they should be.

Simon Hornblower does justice to both aspects of the historian's work: to the rigor of his method, with his careful use of evidence and his concern for precision and accuracy, and to his artistic genius as a writer, skilled in the use of language and structure and clever in his choice of devices to set a mood or convey an interpretation. The presence of both of these qualities has confused many readers over the centuries, often leading to overemphasis on one or the other. It is to Hornblower's credit that he recognizes the tension between them and shows that this tension results to a large extent from Thucydides' role as an innovator in the development of the study of history and

the writing of prose. Thus, there is a tension in Thucydides' attempt to be comprehensive even while he is selective, between his objectivity and subjectivity.

There has long been a problem for readers in knowing how to understand Thucydides' reports of speeches. Are they attempts at reasonably accurate accounts of what was said, free inventions of the author, or something in between? Here, again, Hornblower pays full attention to both aspects of the argument but very sensibly insists on a hard core of historicity for the speeches. This has important methodological implications, for it means that the speeches may not safely be used as evidence for Thucydides' own thoughts, which must be sought in statements made in his own voice.

Some of the more interesting sections of the book deal with Thucydides' opinions. Hornblower depicts him as a conservative, eager to avoid civil strife, which he identified with democracy. This idea made him favor oligarchy, although the Athenian democracy was itself remarkably stable. Hornblower attributes Thucydides' failure to notice that stability to "the prejudices of his own [aristocratic] class" (p. 165) and his hatred for such demagogues as Cleon. On the difficult question of the historian's views on morality, Hornblower makes a persuasive case that Thucydides was a man of strong moral conviction but not a moralist: "he does not try to improve the reader directly, or distribute praise or censure on every page" (p. 189). But in no way was he amoral or morally neutral.

Hornblower is a great admirer of his subject's work and contribution, but in one respect he is most critical: Thucydides' work was "profoundly damaging" because its great influence "ordained that history should henceforth be primarily a matter of war and politics" (p. 30). He does not explain why Thucydides departed from the broader definition employed by Herodotus. Perhaps Hornblower attributes this departure to a soldier's lack of imagination. In another context he says that "Thucydides the general has got the better of Thucydides the poet" (p. 175). Another possibility is that Thucydides believed that war, politics, and international relations are the matrix in which civilized life takes place and that a serious and vigorous understanding of them has a particular importance.

DONALD KAGAN
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MICHAEL GRANT. *The Rise of the Greeks*. (History of Civilisation.) London: Weidenfeld and Nicolson. 1987. Pp. xvi, 391. £17.95.

Michael Grant has edited, translated, and written close to fifty books. This work covers the period from the collapse of Mycenaean civilization around 1100 B.C. to the Ionian revolt just after 500 B.C. Chapter 1 provides a survey of the period, and chapter 2 is devoted to Athens. Thereafter, the arrangement is primarily geographical. Grant casts a wide net, from France and Spain in the west to Ionia in the east and from southern Russia in the north to North Africa in the south. He claims to have examined developments in about fifty states with briefer notes on a dozen or two others. Three appendixes survey Greek relations with other peoples of the ancient world. There are two chronological tables, a brief bibliography, a selection of high-quality plates, thirteen conveniently placed maps, and a full index.

The amount of space devoted to each of the many city-states is largely dictated by the availability of the evidence. Grant usually begins by relating the mythological origins of a particular state and then moves on to a brief synopsis of its historical development, including what is known of its governmental and societal institutions, its art, architecture, literature, coinage, philosophy, and religion. Frequently much of the discussion centers on the most famous citizen or citizens of a particular polis, ranging from poets such as Homer of Chios, or political figures such as Pheidon of Argos, to philosophers such as Pythagoras of Croton. Grant is at his best when dealing with these individuals, particularly literary figures, whose works are nicely summarized for the reader, and in his brief discussions, interspersed throughout, of various potters, artists, and architects as well as individual works of art such as the Chigi Vase or the Vix Crater.

In tracing the course of events throughout the entire Greek world in a straightforward and easily readable style, Grant might have performed a useful service, but the volume is marred by a number of shortcomings. His insistence on referring to the tyrants as dictators serves no useful purpose and is misleading, as is his contention that the Spartan *helots* were serfs, not slaves. But these are minor matters.

Anyone familiar with the period knows that it is not an easy one to deal with. The evidence, whether literary, archaeological, or a combination of both, is rarely conclusive and has been variously interpreted. Some of the interpretations offered here are not supported by the evidence. Urbanization, dated to the tenth century B.C. generally, and even earlier, ca. 1075 B.C. at Argos, is placed far too early. Other interpretations, though perhaps defensible, no longer command the authority they once did: that Homer authored both the *Iliad* and the *Odyssey*; that the Etruscans were indige-

nous to Italy; and that the cult of Dionysus was introduced into Greece from Thrace. In fairness, Grant usually (though not always) acknowledges that other interpretations have been put forth. Inclusion of different interpretations would have been a welcome addition to his views on trade, which is overemphasized throughout. It is deceiving to speak of "businessmen" at Cumae and Pithecusae in the eighth century B.C. (p. 311), and not everyone would agree with the proposition, baldly stated here, that the primary reason for Greek colonization was trade. The overemphasis on trade leads to some causal connections that are, at best, fanciful. We are told that Miletus was so heavily dependent on trade with Sybaris that the destruction of Sybaris gave rise to economic problems at Miletus and that these in turn contributed to the revolt against Persia, and, when Hippias ran into difficulties at Athens, "it was largely because of an economic recession caused by the Persian advance [into Thrace] under Darius I" (p. 62). Grant is certainly entitled to his opinions, but, in a volume aimed at the general reader, it is unfortunate that they are sometimes presented as facts.

As one who has enjoyed and profited from many of Michael Grant's books over the years, I am sorry to say that *The Rise of the Greeks* is a disappointment.

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RICHARD GARNER. *Law and Society in Classical Athens*. New York: St. Martin's. 1987. Pp. viii, 161. \$27.50.

Richard Garner's book, frankly addressed to the classicist but accessible to any interested reader, attempts to show the fit between the shape of Athenian legal practices and the pervasive patterns of conduct and value observable elsewhere in Athenian life of the fifth and fourth centuries B. C. It should come as no surprise that he is able to demonstrate convincingly how the Athenian court system, in language, procedure, and informing spirit, was yet another arena in which Athenians strove competitively to outdo one another.

Apart from the homicide court, which retained its ancient and sacred character, Athenian legal institutions grew up with the emerging and imperialist democracy. They reflect an (arguably) more secular inspiration—Athenian laws came from men, not the gods or a divinely inspired lawgiver—and they restrained individual action with sanctions neither divine nor familial. Litigation is an example of how democracy, with a value system still aristocratic and more competitive than cooperative in nature, attempted to check civilly dangerous self-assertion while providing at the same

time another opportunity for seeking personal honor and glory.

In view of Garner's own sense of cultural isomorphism, it is a little difficult to understand his emphasis on the secular spirit of the nonhomicide courts. Why should this one sphere be so set apart in a society where the gods were omnipresent? More problematic is his conclusion that laws and suits pervaded Athenian life, not any abstract ideal of justice or of a legal system as a good in itself. For Garner the legal system only matured in the fourth century, although a survey of attitudes evident in fifth-century drama suggests a development in the direction of abstract idealism. Garner argues that the critical event was the oligarchic upheavals in the last years of the Peloponnesian War. This trauma created a more urgent sense of constitutionality and legal value.

Without wishing to minimize the importance of the Thirty Tyrants (Xenophon is a powerful witness here whom Garner does not cite), a consideration of earlier Athenian tyranny could prompt a different exegesis. Athens stood apart from most Greek cities in going through a period of tyrant rule later and shorter than that in other cities in the seventh and sixth centuries. When finally in power, the Peisistratids ruled in accordance with the current constitutional apparatus. The Athenians, in short, seem to have had a developed constitutional sensitivity favoring a system of law over personal power well before 403 B. C. The Athenian institution of the *graphe paranomon* for suits alleging constitutional violations is a creation of the fifth, not the fourth, century, which Garner ought not to gloss over as easily as he does. Seen in this context, perhaps it is possible to offer a closer explanation for the notorious litigiousness that was such a peculiar feature of Athenian life. Appeals to the agonistic temper of Greek life, such as Garner reasonably makes, are still not sufficient; they could apply to any Greek city.

Finally, it is not clear, in a book otherwise admirably straightforward, how or why the distinction between legal wrangling and "legality" or "justice" is as sharp as Garner appears to want. And it does not help to cite fifth-century appeals to the fear of apprehension or punishment as betraying the weakness of any sense at that time of a legal standard persuasive in its own right. For fears of legal retribution, it is possible to imagine, can coexist in the same individual, let alone the same society, with a superior understanding of the value of being law-abiding. To borrow from the proverb, salutary fear of the law may be the beginning of wisdom.

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GRAHAM SHIPLEY. *A History of Samos, 800–188 B.C.* New York: Clarendon Press of Oxford University Press. 1987. Pp. xviii, 352. \$72.00.

"A full history [of Samos] has had to wait for an age in which it is positively fashionable to emphasize the diversity of the Greek world through regional studies" (p. vii). So argues Graham Shipley of his book. Although the assessment is surely correct, the author's success owes much to another fashionable characteristic of ancient history, namely, recognition of the diversity of available evidence. Keen awareness of the interplay of various factors has produced a work that is far more than a regional study of one island little more than a mile off the coast of Asia Minor. In addition to being a model for other similar studies, the history provides a window on major developments during six hundred years of Greek history.

The history extends over the four main phases of Greek antiquity: the late Dark Age, the archaic, classical, and Hellenistic periods. Such a broad sweep is partly responsible for the variety of data, because the available types of information differ significantly in each period. Literary sources, archaeological data, epigraphical materials, and other historical sources are employed consistently and confidently. Shipley is aware of the problems of the traditional evidence, and he regularly goes beyond these sources. Recognizing the importance of geography to the Samian story, he undertook a regional survey to gather data. A catalogue of sites in Samos and the Peraea is a valuable addendum to the narrative. Theoretical argument also plays a significant role in the study. In fact, Shipley has produced a fine systems analysis of Samos, demonstrating the impact of change on every aspect of Samian culture. While appreciating the importance of modern theoretical models, he avoids the mistake of reading modern notions into the ancient mentality. As he says of Polycrates, "It would be a mistake to credit him with economic ideas he could not have formulated" (p. 83). Finally, Shipley employs comparative analysis that cuts across periods as well as geographic areas. (The course of Samian history, for example, is placed in sharp contrast to that of neighboring Chios.)

The reach of the story matches the range of data with the situation of Samos set against the larger background of Greek development. The role of other states—Sparta, Athens, Persia, and, later, the Hellenistic kingdoms—regularly provides the backdrop for the specific events of this one part of the Greek sphere. The balance between general and specific is deftly judged and maintained. What is more, Shipley turns to the microcosm of individual social history when evidence permits, offering at least a taste of what Eugen Weber,

historian of France, describes as the small change of great events.

Samos' story proves to be significant to an understanding of the larger Greek world. The island seems to have been one of the earliest communities to achieve the corporate self-consciousness that defines the polis. During the tyranny of Polycrates, it held a commanding economic and cultural position that was emulated by other states. Even in a dependent role, this island polis was an exceptionally valuable ally or subject—to Athens in the fifth and fourth centuries and to Alexander's successors after 323 B.C.

In his argument Shipley is thorough and careful; he asks sensible questions. Methodical presentation of data is followed by sensible conclusions. Surely there will be disagreement with specific decisions, but there seem to be few egregious errors. (It was a surprise to find Ptolemy listed with Cassander, Lysimachus, and Seleucus as victors at Ipsus on page 174.)

The book is technically pleasing. The illustrations are both useful and attractive; there are very few typographical errors; the more technical material is placed in nine appendixes; the index is full and accurate. It is fully evident that the subject has won the heart of the author; Shipley's account does credit to the past of "one of the most beautiful places in Greece" (p. 5).

CAROL G. THOMAS
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MEDIEVAL

R. I. MOORE. *The Formation of a Persecuting Society: Power and Deviance in Western Europe, 950–1250.* New York: Basil Blackwell. 1987. Pp. viii, 168. \$27.50.

In this brief and provocative essay, R. I. Moore argues that during the eleventh and twelfth centuries there took shape in Western Europe what he calls a persecuting society. In this period "deliberate and socially sanctioned violence began to be directed, through established governmental, judicial and social institutions, against groups of people defined by general characteristics such as race, religion, or way of life; and that membership of such groups in itself came to be regarded as justifying these attacks" (p. 5). Heretics were vigorously pursued, Jews burdened with the stereotype that they were blaspheming and sacrilegious enemies of Christ, lepers segregated from the rest of the population, and such groups as homosexuals and prostitutes treated with a new rigor.

Moore contends that these developments were not accidentally related, nor were they the result of a simple response to growing numbers of her-

etics, Jews, and lepers. Moreover, the actions taken against these groups by the authorities in the twelfth century were not caused by popular hostility; indeed, such hostility scarcely existed. What prompted the development of a zeal for persecution was a transformation of the world view of the directors of European society. Heretics, Jews, and lepers, among others, were the victims of a general social reclassification carried out by the ruling elites, which precisely defined certain individuals as members of outgroups and classified those groups as dangerous.

To account for this change in the mentality of the ruling elites, Moore draws attention to two factors. First, the profound changes that affected twelfth-century Europe—the creation of the seigniorial economy, the rise of a money economy, and the growth of new governmental institutions—all led to an increasing gulf between rich and poor. Therefore, the development of effective means of social control was very much on the agenda of the rulers of twelfth-century Europe. Second, the rising importance of a new, literate, clerical element in the twelfth-century governing institutions was intimately connected with the development of a persecuting society. For these men, the creation of a system of persecution was an integral part of their effort to consolidate, often in the face of serious opposition, their grasp on the levers of power. Persecution became at one and the same time a method for suppressing resistance to authority and legitimating that same authority.

Moore's essay is one of the more provocative and stimulating books that I have read in recent years. It deserves a large audience; the issues it raises are important and should be widely debated. Not everything, however, that Moore argues will convince all readers. I doubt, for example, that his argument that the creation of a persecuting world view helped the clerical elites gain the upper hand over their rivals for positions of influence in the new governing institutions of Western Europe will meet with general acceptance. Moore also, rather curiously, leaves the concept of persecution largely undefined. I therefore found myself wondering if it makes sense to treat as manifestations of a single impulse both public health measures directed against lepers, who, after all, did suffer from a disease (whether it was really leprosy or not) believed to be contagious, and the treatment of Jews, who were often stigmatized for behavior, such as ritual murder, in which no Jew ever engaged. Nevertheless, despite the problematic nature of many of his arguments, Moore has succeeded in making it clear that the development of a persecuting mentality was an integral, and central, aspect of the transformation

of European society in the eleventh and twelfth centuries.

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JAMES A. BRUNDAGE. *Law, Sex, and Christian Society in Medieval Europe*. Chicago: University of Chicago Press. 1987. Pp. xxiv, 674. \$45.00.

In the study of medieval norms of sexual behavior, this book marks a milestone. Hitherto, those seeking information about medieval legal and moral teachings about sex have had to consult old and incomplete surveys or to ferret out specialized studies in hard-to-come-by journals. Henceforth, they will look with gratitude and appreciation into James A. Brundage's encyclopedic survey.

The most impressive quality of his book is its comprehensiveness in every sense of the term. Chronologically, Brundage moves in twelve chapters from the ancient Middle East, through classical antiquity and the Middle Ages, up to the early Reformation and the Council of Trent. For each of the major chronological divisions, he summarizes the character of legal thinking and then reviews ideas, norms, and counsels bearing on all kinds of sexual or sex-related behavior: interpretations regarding the purposes of sexuality, marriage and remarriage, eligibility for and impediments to marriage, consanguinity and affinity, concubinage and polygamy, clerical marriage, divorce, fornication and adultery, licit and illicit sexual acts inside and outside of marriage, prostitution, homosexuality, masturbation, and still other topics. In the first five chapters, up to 1140, and again in the two chapters covering the years 1348 to 1563, he largely summarizes the work of other scholars. The middle three chapters survey the classical age of medieval canonical jurisprudence, represented by the *Decretum* of Gratian, the compilations of decretals (papal letters), and innumerable glosses and commentaries. Here, Brundage principally relies on his own research, much of it based on unpublished manuscripts. These he has found in numerous European and American libraries.

The range of publications he cites, some of them found in the most unlikely journals, is no less impressive. The citations in the footnotes are so many that, perhaps unfortunately, he selects only about one-tenth of them to include in the book's appended bibliography. He adds to this rich fare three appendixes: the first showing the distribution by time, place, and content of early medieval legal sources; the second taking issue with Jack Goody's recent interpretation of early medieval

incest prohibitions; and the third illustrating the survivals of medieval canonical terms and concepts in modern American legal codes. Twenty plates give visual expression whether to canonical principles or to sexual behavior.

Of all branches of medieval studies, canon law is perhaps the least controlled for an evident reason. Traditionally, national historical societies have considered that the international law of the medieval church had little to contribute to their national histories; accordingly, with few exceptions, they made no effort to locate, publish, and interpret the many still-extant legal texts. Consequently, Brundage's thorough survey of sexual law, including unpublished texts, is all the more valuable. It is sure to enjoy a long and useful life.

To criticize so large and rich a work seems almost petty, but one remark seems justified. Brundage summarizes the sexual norms of the early medieval penitentials through a kind of game board (figure 4.1), in which the player seeking intercourse must appropriately answer some twenty questions. A wrong answer leads to a square labeled "Stop! Sin!" However bizarre the penitentials may be in modern perspective, ridicule is not the way to decode their meaning. Brundage is well aware that sexual norms are not formed in a vacuum; rather, they will always reflect a society's deepest assumptions concerning the nature of the cosmos and human destiny within it. He should have tried to explore the cultural roots of these strange restrictions. There is a depth in them that mockery will never reach.

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ROBERT BARTLETT. *Trial by Fire and Water: The Medieval Judicial Ordeal*. New York: Clarendon Press of Oxford University Press. 1986. Pp. 182. \$39.95.

With great economy and elegance, Robert Bartlett undertakes to describe the rise, spread, function, and decline between the fifth and the fourteenth centuries of the medieval judicial ordeals. Because such a work requires the control of an irregular group of sources as well as a response to a large and varied body of earlier scholarship, Bartlett really faces two tasks. In my view, he accomplishes both.

Bartlett focuses on the ordeals of fire and water in his account of the early history of the ordeals, because he regards later variations in forms as elaborations of these earliest methods. His conclusions are historical and specific: except for a possibly independent (and in any case uninfluential) Irish procedure, the judicial ordeal was specifically

a Frankish practice (so much for arguments from Pan-Germanism), elaborated under the Carolingians and from them spread to other peoples. Bartlett's argument here has been independently supported recently by Ian Wood and Paul Fouracre (Wood, "Disputes in Late Fifth- and Sixth-Century Gaul: Some Problems," in Wendy Davies and Paul Fouracre, eds., *The Settlement of Disputes in Early Medieval Europe* [1986]).

From the ninth century on, the ordeals were employed by authorities in certain kinds of cases and not others and used against people of certain statuses and not others: for example, often in cases of particular political importance and complexity; in cases to determine sexual purity; later, to support the Peace of God movement and to test religious orthodoxy; often against those who had forfeited their oath-worthiness; against strangers, slaves, and other servile folk; and, always, "as *lex paribilis*, or *apparens*, or *aperta*—the 'manifest proof.' It was a device for dealing with situations in which certain knowledge was impossible but uncertainty was intolerable" (p. 33).

Bartlett then takes up in the fourth and fifth chapters the problem of the demise of the ordeals. During the twelfth century, uncertainty indeed remained intolerable, but new kinds of certainty became possible. Bartlett addresses the demise of the ordeal by examining two distinct groups of earlier explanations, those concerning social change and those concerning changes in belief. Bartlett is rightly critical of explanations that argue for a gradual withering away of the ordeal in the twelfth century as a result of changing social structures or anything else, because he finds more than sufficient evidence for the spread of the practice during the period. He is gracefully impatient with arguments concerning social consensus, since he emphasizes that ordeals were the instruments not of face-to-face communities but of clerical and lay lords with specific coercive powers. Susan Reynolds (*Kingdoms and Communities in Western Europe, 900–1300* [1984]) and R. I. Moore (*The Formation of a Persecuting Society* [1987]) have also argued against "consensus explanations."

In the somewhat more complicated area of changing beliefs, Bartlett defines what ordeals were and were not supposed to accomplish, the antiquity of most of the arguments made in the twelfth century against their validity, and the particular circumstances among twelfth- and thirteenth-century clerical elites that militated against them. Bartlett is emphatic: intellectual support of the ordeals did not wither away; "there was no decline of the ordeal; it was abandoned" (p. 100). People "did not abandon [the ordeal] because it was irrational, it became irrational when they

abandoned it" (p. 86). (So much for irrational and rational models of legal change in history.)

Drawing heavily and competently on twelfth-century ideas of law, sacramentalism, scriptural exegesis, and, especially, newly articulated concepts of canonicity held by clerical elites who were bent on separating themselves from lay authority and its expressions, Bartlett argues that neither increased rationality nor increasingly sensitive theology drove ecclesiastical policy. Rather, policy made by and in the interests of a literate and influential cadre of clerical reformers—reflecting, among other things, the dissolution of the earlier "symbiotic" union of priests and secular rulers—appropriated some particular components of twelfth-century (and earlier) thought in the service of its own agenda. Bartlett devotes a chapter to trial by battle, which, he argues, is distinctive enough to require separate (and equally sensitive and intelligent) treatment.

One test of Bartlett's argument is an examination of what happened after the abolition of ordeals. First, Bartlett reminds us that the actual process of abolition was long and slow. It took most of the thirteenth century. He also analyzes the extension of modes of existing proofs into areas once reserved exclusively for the ordeal: the swift growth of the trial jury in England and a few other places and the rise of inquisitorial procedure and torture.

Bartlett has gotten the history right because he has used two sensible rules for historical scholarship. It is a good rule that, if one element in a culture appears "dramatically alien" (p. 1) among otherwise readily comprehensible elements, we have probably failed to understand both the other elements and the "alien" one as well. It is another good rule that, in order to understand what people in the past thought about something that interests us, we should also examine those things with which they habitually identified it. Toward the end of his study, Bartlett cites the complaints, made around 1219, of Emo of Bloemshof, a well-educated cleric, which Emo used to account for a wave of natural disasters in Frisia. The Frisians, Emo remarked, were great sinners, particularly their clergy, who employed dubious ordinations, alienated church endowments, practiced clerical marriage, and failed to abolish the ordeal of hot iron, "contrary to the canon." For Emo, the ordeal was sinful because it was uncanonical, in exactly the same way that the other elements in his account of immanent justice were uncanonical (so much for the alleged demise of theories of immanent justice). For the scholastic and curial elites of the late twelfth and thirteenth centuries, the ordeal was discredited because of "its lack of harmony with their specialized and novel ideology,

not with the outlook of the great majority" (p. 164).

Bartlett has not explained all of twelfth-century thought, but he has explained one thing very convincingly. His arguments and style should be useful for explaining others.

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PIERRE ALEXANDRE. *Le climat en Europe au Moyen Age: Contribution à l'histoire des variations climatiques de 1000 à 1425, d'après les sources narratives de l'Europe occidentale*. (Recherches d'histoire et de sciences sociales, number 24.) Paris: Ecole des Hautes Etudes en Sciences Sociales. 1987. Pp. 827. 290 fr.

This is not a volume for light reading; it is more like a telephone book. Nevertheless, for dedicated enthusiasts, the work is packed with interesting material. As with many French-language works, it has a useful table of contents at the back, a fourteen-page bibliography, but no index (unforgivable, in my opinion). It is subdivided into an introduction, which reviews sources, their reliability, and the author's objectives; a part 1, dealing with medieval textual sources, item by item, arranged regionally in thirty-eight areas from Wallonia to Istria; a part 2, a catalogue of meteorological events, century by century, including such things as terminological problems and confusions; a part 3, arranged in three chapters, dealing with homogeneous data series, tables of regional temperature and seasonal rainfall, annual tables, regional comparisons for the years 1891–1960, quantification methods, and, finally, a synthesis of the secular variations in climate through four centuries.

Pierre Alexandre, for the purposes of this volume, takes the Middle Ages to run from 1000 to 1425. His definition of Europe is also rather arbitrary but understandable for a French-speaking audience. Europe, astonishingly, excludes the British Isles, the Iberian Peninsula, Scandinavia, Russia, and the Balkans.

Despite these peculiarities, we must take his results very seriously. I, like other palaeoclimatologists, have my own ways of testing the written word—through dendrochronology (tree-ring growth), lake-deposit layers, astronomic cycles, auroral records (proxies for sunspot levels), and so on. According to Alexandre's results, out of 425 years, the following years were established as having the twenty harshest (coldest) winters: 1069, 1077, 1126, 1143, 1150, 1173, 1210, 1219, 1234, 1253, 1276, 1303, 1317, 1323, 1326, 1339, 1354, 1361, 1364, and 1408. Of these I noted that 60

percent fell at or within two years of a peak in the eleven-year sunspot cycle; several were in the season following major eruptions of volcanoes in Iceland, of Vesuvius, or of Etna.

Advocates of climatic determinism in historic events win a powerful ally in Alexandre. The great hardships, famines, and political upheavals of 1150–69, 1190–99, 1310–19, and 1340–49, including the Black Death plagues of 1348–50, all coincide with intervals of summers marked by cold, wet conditions or years of extreme drought, often accompanied by very harsh winters. The big departures from the average were often in successive years. Tree-ring records from North America and North Africa show those same disturbed decades. They are emphatically nonrandom.

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CLIVE FOSS and DAVID WINFIELD. *Byzantine Fortifications: An Introduction*. (Unisa 1986, number 22.) Pretoria: University of South Africa. 1986. Pp. xxvii, 298. \$35.00.

From the third to the fifteenth century, the Byzantine empire struggled constantly to defend its centers of population, commerce, and administration against a variety of external threats that changed in intensity and technological sophistication. The Byzantine response to those threats is nowhere more clearly demonstrated than in the fortifications erected to protect key sites. Clive Foss and David Winfield here combine their knowledge of history, archaeology, and architecture to make clear the intimate relationship between the technical aspect of fortification and the broader history of the era.

In part 1, Foss and Winfield outline the general features of Byzantine military architecture and the historical context in which they developed. The authors also attempt, without great success, to relate the development of Byzantine fortification to developments in the medieval West. Part 2 consists of a detailed discussion of the two most important Byzantine fortification systems: the walls of Constantinople and of Nicaea. In part 3 the discussion is extended to the Byzantine fortifications of Asia Minor.

The authors discuss in a systematic manner the form and significance of selected fortifications erected in various periods. They first sketch the history of the site using contemporary written evidence, memorial inscriptions, and the research of modern scholars. They then discuss the defensive techniques prevalent in the period under

consideration with special attention to new military technology (the *trebuchet*, for example) and how those weapons were accommodated in the construction of defensive works. There then follows a detailed discussion of the materials, masonry styles, and construction techniques employed at the site. The authors successfully demonstrate that examination of building materials, techniques of reinforcement, and other architectural details makes possible both the dating of a fortification and some conclusions regarding the nature of the threat and the Byzantine military response at that time and place.

The technical details required to establish the chronological schema may befuddle readers without specialized training because the book contains no glossary of such basic terms as *ashlar*, *cloissoné*, and *voussoirs*. The text, however, is supplemented by a generous selection of photographs, maps, plans, and illustrations that are remarkably clear and that effectively support the points made by the authors. A good bibliography and an adequate index are also provided.

Foss and Winfield succeed admirably in the difficult task of combining several disciplines to produce in one volume both an introduction to Byzantine fortifications for the general reader and an interpretive study for more advanced students of Byzantine history, military affairs, and architecture. We now have for the first time a comprehensive study of Byzantine fortification that integrates the physical evidence with the written sources to enhance our understanding of a particularly important aspect of Byzantine civilization.

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R. J. SMITH. *The Gothic Bequest: Medieval Institutions in British Thought, 1688–1863*. New York: Cambridge University Press. 1987. Pp. xiii, 231. \$39.50.

R. J. Smith has examined the manner in which medieval English institutions were discussed by historians, political theorists, clergy, and practicing politicians from the revolution of 1688 through the Reform Bill of 1832. The larger issue in his work is the manner in which history provided a framework for public discourse before professionalization of the discipline.

The Glorious Revolution rendered ineffectual the seventeenth-century theories of the relevance of the medieval inheritance, such as immemorialism, Gothicism, and the Norman Yoke. Furthermore, the emergence of abstract political theory tended to displace genetic or historical theorizing. What took the place of these earlier approaches

was the feudal explanation of the law, the constitution, and the church. In the process the Middle Ages became viewed as a kind of warehouse of useful political examples rather than as a deposit of normative imperatives. As the century passed, the Scottish writers drew the Middle Ages into their various patterns of conjectural history in which the feudal interpretation loomed very large in not only English but also human history. Furthermore, all of these developments made clear that constitutions and other institutions changed and modified over time.

Edmund Burke reasserted in a modified form the earlier Whig interpretation of the Middle Ages, but even with Burke the medieval inheritance was no longer normative. By the turn of the century, however, radicals such as Major John Cartwright were reasserting a new kind of immemorialism, claiming that England did have an unchanging constitution. A number of the Romantic writers and historians also looked in a normative manner to the Middle Ages. By that point there had arisen the ironic situation of early nineteenth-century Tories defending the modern settlement of 1688 and many radicals attempting to use the medieval inheritance as an argument for change.

The author has read very extensively and is clearly familiar with a wide-ranging set of sources. His presentation of materials and his arguments, however, are so compact and rely so heavily on the reader's equal familiarity with both primary sources and secondary works that they often prove difficult to follow. The book reads somewhat like a common room conversation where the disputants have been talking so long with each other that no one needs to explain individual positions. Authors of great complexity such as David Hume, John Millar, and Henry Hallam are considered in only a few pages with very brief quotations, and in some cases none, to support the argument or the assertion of the author. Nonetheless, Smith can and often does make very shrewd comments, such as those on Burke and on the use of history by Oxford Movement authors. One must regret that he did not write either a longer book where his evidence and arguments might be fleshed out or a shorter one in which he considered a particular period in more detail. Nonetheless, he has provided an excellent point of departure for future considerations of the subject and has mapped the intellectual landscape.

One minor correction should be noted. The first use of the term "feudalism" was not in the 1830s as Smith suggests, following the *Oxford English Dictionary*, but in the 1790s by Scottish writers, as Eric Hobsbawm discovered. (See Eric Hobsbawm, "Scottish Reformers and Capitalist Agriculture,"

in E. J. Hobsbawm *et al.*, eds., *Peasants in History: Essays in Honour of Daniel Thorner* [1980].)

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RAYMONDE FOREVILLE and GILLIAN KEIR, editors. *The Book of St Gilbert*. (Oxford Medieval Texts.) New York: Oxford University Press. 1987. Pp. cxiii, 385. \$96.00.

Gilbert of Sempringham, who lived from about 1083 until 1189 and who founded the only native English religious order, stands out as a striking figure among the holy men of medieval England. He was canonized in 1202, and the *Book of St Gilbert* contains the texts assembled at that time, including his *Vita*, the letters concerning the revolt of the lay brothers, the *Canonizatio*, the letters concerning the canonization, and two collections of miracles. These texts are studied in the introduction, which also gives an account of Gilbert and the history of his order, which survived, in greatly reduced circumstances, until the dissolution in 1538–39.

Gilbert was a distinctive character who combined vision and courage with a sense of humor and a measure of self-will. He is more than once described as *hilaris* and *iocundus*, and, when he was in London awaiting trial for having supported Thomas Becket, he bought some tops to amuse his frightened companions. He was helped on this and other occasions by Henry II, who is said to have attributed the status and success of his realm to Gilbert's prayers and presence. Even after his death, Gilbert remained feisty, and once in a vision he refused to help a nun who had called on the devil, saying that she now belonged to the devil rather than himself.

The order of Sempringham in 1200 included ten so-called double houses of men and women and three houses of men. There were said to be fifteen hundred female members, who lived according to the Rule of Benedict, and seven hundred men, who followed the Augustinian Rule. The *Book of St. Gilbert* sheds much light on the new types of religious institutions in the twelfth century and their members, especially the lay brothers and their discontents. It also constitutes one of the first official dossiers for canonization and shows the concern of Innocent III for reliable procedures and witnesses.

Although the translation is for the most part smooth and accurate, some words are translated inconsistently, and a few passages are unclear. Letters patent were open rather than unsealed, and the troublesome phrases on pages 28, 34, and 42 surely mean that Gilbert did not confuse

worldly goods with his heavenly reward, that the nuns should desire God (rather than vice versa), and that the fruit of Gilbert's work should be harvested during his lifetime (the "other condition"). The editors occasionally underestimate the extent to which the texts were revised to meet the needs of the order, as in the letter of Alexander III cited in the *Vita*. I put less trust than they do in the accounts of Gilbert's visit to Cîteaux in 1147 and the revolt of the lay brothers, who seem to me to have had legitimate grievances. But these are matters of opinion and are among the many interesting points raised by this remarkable work.

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ROBERT C. STACEY. *Politics, Policy, and Finance under Henry III, 1216–1245*. New York: Clarendon Press of Oxford University Press. 1987. Pp. xii, 284. \$57.00.

Robert C. Stacey's knowledge of unpublished thirteenth-century English financial records, of published chancery records, and of studies by young British scholars provides him with new materials for depicting Henry III's reign. Previous accounts based chiefly on Matthew Paris's biased and inaccurate *Chronica Majora* have long needed correcting. In this new study, Stacey examines the intertwining of personalities, politics, and royal finances from Henry's accession until 1245 and looks for clues to the king's later crises. He finds that Henry III's troubles did not lead directly or inevitably to the baronial rebellion, for reforms instituted between 1236 and 1239 put his government on a sound footing until 1245. Those years of reform form the core of this book.

Stacey's organization is topical within a larger chronological framework, a compromise between topical and narrative patterns that is not always clear to the reader. He begins with an account of Henry III's early years, which shows how factionalism under Hubert de Burgh's justiciarship forestalled financial innovation. The brief period of Poitevin domination, 1232–34, was no more successful in curing the king's chronic poverty. Chapter 2 details the king's effort between 1236 and 1239 to increase his profits from the shires and demesne manors and control the profits of his sheriffs. Chapter 3, which might have more effectively preceded chapter 2, examines the council members who instituted these financial innovations. Here the book's hero emerges: William de Raleigh, whom Stacey sees as "the guiding spirit behind these reforms" (p. 104) and whose fall from influence marks the end of them. Raleigh

emerges from these pages as more significant than ever. Not only a leading judge but also, thanks to S. E. Thorne's studies, the likely author of the legal treatise attributed to Henry de Bracton, Raleigh was the backbone of Henry III's council in its most effective phase.

Chapters 4, 5, and 6 examine the new council that followed Raleigh's fall and its approach to royal finances. The council under Raleigh's successor, Stephen de Segrave, quickly abandoned the financial reforms of 1236–39, and a drop in royal revenues from the shires resulted. The king then needed new sources of income, and he turned to exploitation of the Jews, the chief subject of chapter 4. Henry III's tallages of the Jews in 1241–42 and 1244–45 "broke the financial backbone of the English Jewish community, and permanently reduced its financial value to the Crown" (p. 154). Chapter 5 treats Henry's spending of the revenues raised by his financial reforms on preparations for his military campaign of 1242 to recover Poitou. Here Stacey concludes that, despite the expedition's costs, "Henry was not critically short of cash in 1244 and 1245" (p. 200), suggesting that the earlier reforms were still strengthening him financially. Chapter 6, an account of royal yearly receipts and expenditures between 1240 and 1245, necessarily repeats material from earlier chapters. Stacey estimates that, although the government had a deficit for three of the five years, Henry's financial resources were greater than previous scholars have supposed.

The concluding chapter looks at Henry III's response to his defeat on the Continent, to measures taken to pay debts, and to their consequences for the rest of his reign. The year 1245 was "a year of distinct financial crisis" (p. 244), which the king could not overcome because he lacked counselors to manage his Parliament and secure substantial aid. Instead, before the barons would consent to new funds, they demanded redress of grievances, concessions, and changes centering on what they perceived as royal threats to property rights. Henry III's inability to raise adequate revenues after 1245 forced him to turn to fiscal expedients, which "raised much more political annoyance than expendable cash" (p. 257).

Stacey's mastery of administrative records enables him to replace Maurice Powicke's venerable but too Whiggish version of Henry III's reign in the years before the baronial rebellion. Stacey's construction of a coherent account based on incomplete financial records and terse royal letters is admirable. Although historians have long been rewriting the history of the Norman and early Angevin kings, basing their work on nonnarrative

sources, Stacey must be hailed a pioneer in applying this method to Henry III.

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MICHAEL BENNETT. *Lambert Simnel and the Battle of Stoke*. New York: St. Martin's. 1987. Pp. vii, 157. \$29.95.

The quincentenary of the battle of Bosworth prompted the British publisher Alan Sutton to commission several short books on the coming of the Tudors. Like its predecessors in this series, including Michael Bennett's *The Battle of Bosworth* (1985), this book has been designed for a popular audience. Jacket, endpapers, maps (two), illustrations (twenty-eight, or one for every four pages of text), and Bennett's slightly breezy prose style are meant to appeal to museum-shop crowds. The substance of the text, however, as much as the notes and appendix (consisting of extracts from eleven original sources, including several translated by the author from Latin and French) mark his work as a serious attempt to explain the origins of the Tudor regime.

On June 16, 1487, at Stoke Field near Newark an army royal of about fifteen thousand men under the command of the duke of Bedford and the earl of Oxford—King Henry apparently looked on from the safety of a steeple—defeated a force of about eight thousand, including (in addition to contingents of half-naked Irish kerns) some two thousand German mercenaries paid for by Margaret of Burgundy, the sister of Edward IV and Richard III. The rebel commander was Margaret's nephew, the earl of Lincoln, whom Richard III might have designated the Yorkist successor. Stoke was York's last stand, a courageous, hard-fought challenge to the verdict at Bosworth. Stoke wrote *fini* to the Wars of the Roses; it was the last time an English king had to defend his title in battle.

Had Lincoln won, not he but Lambert Simnel, an Oxford schoolboy, would have become king, for such was the Yorkist scheme. Simnel, an impostor, was standing in for the man who then held the best claim to the throne, Henry VII's prize prisoner of state—Edward, earl of Warwick (b. 1474), the son of Richard III's brother George, duke of Clarence (d. 1478).

Bennett reexamines Simnel's progress to Stoke in order "to see how dynastic uncertainty and civil strife created a political culture in which imposture flourished" (p. 13). In this self-set aim Bennett succeeds admirably; although much of the Simnel story remains obscure for lack of evidence, the author brings to the fore the relevant genea-

logical and political data. On one point the known facts do not necessarily support his conclusion. Bennett thinks that Henry VII's order for the deprivation and removal to Bermondsey of the queen dowager, Elizabeth Woodville, had something to do with the Simnel conspiracy (pp. 50–51). S. B. Chrimes once demonstrated why such a presumed connection "is by no means certain" (*Henry VII* [1972], p. 76, n. 3).

The important connection in Simnel's case lies elsewhere, in Ireland. At Stoke, Henry VII halted a foreign invasion that sought to use Ireland as a Yorkist base for control of the kingdom. Bennett's discussion of the Irish connection poses a fundamental problem of perspective. His traditional schema places Ireland in the background (pp. 63–68), making Stoke the focal point of an English dynastic story. Perhaps the Simnel episode should be viewed the other way around, from the Irish center of these events. Simnel was the puppet of the Yorkist wing of the "Home Rule" Anglo-Irish lords. In Ireland, Home Rule and the cause of the Yorkists had been one ever since the lieutenancy of Richard, duke of York (1449–60), when a parliament at Drogheda had declared Ireland independent. Simnel's pose as Edward VI, Clarence's son and heir, drew deeply on this Yorkist and Anglo-Irish tradition: the pretender was crowned in Dublin, Clarence's birthplace, where Clarence was remembered as the "countryman and protector" of the Anglo-Irish proponents of "Home Rule." Simnel's title advertised just how far the Irish Yorkists were prepared to go to secure Clarence's inheritance for "their" king: Simnel was to be "King of England, France, and Ireland" instead of the usual "King of England and France, and lord of Ireland," a point that Bennett misses. The issue at Stoke transcended the fate of Lancaster and York. Was it not really a question of the relation of Ireland to England?

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CHRISTOPHER ALLMAND. *The Hundred Years War: England and France at War, c. 1300–c. 1450*. (Cambridge Medieval Textbooks.) New York: Cambridge University Press. 1988. Pp. x, 207. Cloth \$34.50, paper \$9.95.

It is an act of courage to write a short book on the Hundred Years' War. It is yet more courageous, and commendable, when the author sets as his goal no less than showing "how war over a period of 150 years or so, affected developments and changes in two of Europe's leading societies, those of England and France" (p. 2). The scholarship to be surveyed and summarized is vast (as the twenty-

page select bibliography suggests). Almost any aspect of English and French society could be related in some way to the war. The sweep of the book is thus responsible for both its strengths and its occasional weaknesses.

The opening chapter gives an admirably lucid summary of the causes and progress of the war itself. This may be the most readable short survey of the campaigns yet written. In the following six chapters, Christopher Allmand interprets the war in a variety of contexts: attitudes to war, both popular and learned; the conduct of war; the institutions of war; the socioeconomic effects of war; the connection between war and nascent nationalism; war and literature. Each of these topics is well chosen, and each chapter would give a student (at any level) a good introduction. It is especially refreshing to see Allmand move the debate over the costs of war beyond the confines of the K. B. McFarlane and M. M. Postan debate about bullion and to consider, instead, the effects of war on the economy of Europe. Similarly, his interest in using literature as historical evidence gives him a usefully wider base for understanding attitudes toward war.

In a few cases, topics or connections seem missing. No connection, for example, is drawn between the war and the Jacquerie of 1358. Equally surprising, there is no discussion of the credit arrangements that enabled the English crown to consider war on a grand scale in the late thirteenth and early fourteenth centuries, the decades that led into the Hundred Years' War as it is traditionally dated. We are told that medieval states "scarcely used the credit system to start wars" (p. 106), a statement that would have surprised a number of English kings and the Italian *societates* that served as their financiers. The chapter on literature makes almost no use of the voluminous complaint literature in England that so often took as its targets government actions (often none too gentle) to collect war funds.

It is troubling in a different way to be told that war was not a matter of greed and anarchy but an event "coming to be seen as an instrument of state, to be organized by the king for the common good of his people and country" (p. 52). Should we believe government statements in any age that each war in turn is for the common good? Mercenaries seem to get the blame for devastation (p. 73), though nobles, we learn, regulated their conduct through the chivalric code (p. 112). Does the evidence suggest that knights were fairer, less destructive in war than the lesser breeds who fought without the law of chivalry? War was directed mainly at civilians (p. 55), and chivalry hardly took cognizance of those below the knightly level.

But such criticisms (and those every specialist can bring against a small general book on a broad theme) should not reduce the sense that this is a valuable book. It will be widely used as a starting place for the study of this significant period of Anglo-French conflict.

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RICHARD W. KAEUPER. *War, Justice, and Public Order: England and France in the Later Middle Ages*. New York: Clarendon Press of Oxford University Press. 1988. Pp. x, 451. \$78.00.

A great deal of detailed research has been done in the last twenty years or so, by both English and French historians, on the question of the fourteenth century as a period of relative economic, social, and political decline when compared with the great achievements of the thirteenth century and on the origins and development of the protracted period of conflict popularly characterized as the Hundred Years' War. But there have been few attempts to see these major historical events as closely interlinked and as having a common origin in the historical development of the state in England and France from the eleventh century.

In this excellent book, Richard W. Kaeuper argues that the intensity and scale of the wars involving England and France after 1290 represented a completely new departure from earlier events and that the impact of enhanced warfare acted as a catalyst in determining the pace and direction of the growth of state power in both countries and, beyond this, was a significant factor in the economic, political, and cultural life of northern Europe as a whole.

The detailed hub of the book is the period from 1290 to 1360, the year of the Peace of Bretigny. But Kaeuper reaches before and beyond this half-century or so (years that coincide with his specialization) to argue that, in both England and France, the period after 1290 represented a distinct break with the High Middle Ages and marked the beginning of a major new historical era that endured in England until the middle of the seventeenth century and in France up to the revolution.

Kaeuper examines the interaction of law and war after 1290 from four principal directions. The first two chapters of the book examine the dichotomous tendencies of late medieval kingship: on the one hand, the king as a warlord and *primus inter pares* among his aristocratic coterie and, on the other hand, the increasing tendency for kings to appear as law makers and law enforcers in their own kingdoms and in that sense as potential enemies of their aristocratic supporters.

Kaeuper outlines the development of state power in England and France after the Conquest and argues that Edward I was in a much stronger position than his Capetian rivals to wage a sustained war, but, nevertheless, the escalation of war in the fourteenth century brought serious problems for both countries. In England the cost of financing internal and external wars strained the outwardly sophisticated machinery of government to breaking point, producing serious internal unrest by the middle of the fourteenth century. By 1360, Parliament had veered away from its earlier course toward representative government to become, increasingly, a sop to Edward III's demands for war taxation and subsidies. In France, he argues, the war with England produced equally negative results; the period from 1380 on witnessed the effective collapse of central government and prepared the way for the English invasion under Henry V.

Chapter 3 explores the phenomenon of chivalry in the context of war and looks at the serious problem of the control of aristocratic factions. In both countries, kings strove to control the sort of heroic violence that the code of chivalry demanded, but at the same time they were obliged to depend on it in time of war. It should not be forgotten, after all, that the essence of the English quarrel with France over sovereignty in the English-held areas was both feudal and legal. The tendency of the *parlement* to view the problem in legal terms was met with a proportionately violent military response from the English kings.

The long ultimate chapter looks at the way in which war affected popular representation and public opinion on both sides of the Channel after 1290. Kaeuper concludes that, in both countries, the war slowed, and radically changed, the direction of the development of representative institutions. In both countries the governments conceded a degree of autonomy in the provinces in return for the consent, and the means, to prosecute the war. Such a policy, it may be said, represented a far more rational development for France, a large and diverse country, than for England, as events after the English expulsion from France in 1453 bore out. In the long term, indeed, the period of Anglo-French warfare was undoubtedly more beneficial to the French than to the English. Against Kaeuper's rather negative view of the effects of war in general, it could be argued that, for the French, English intervention brought considerable benefits, notably the growth in influence of the *parlement* and the increasing ability of Valois propagandists to portray France as the wronged party in the conflict. The crucial element in this scheme appeared in the shape of Joan of Arc at Orléans in 1429: France was to be

united in its critical hour by the direct intervention of God's agent.

So, while French propagandists might justify their absolutist tendencies by pointing to English aggression, English constitutionalism, so praised by Victorian Whigs, had no such justification. Kaeuper views constitutionalism as a product of the reaction of English political society to a combination of royal overextension and the high costs of war.

This subtle and illuminating book will be valued by historians and students alike, not least for its tendency to stimulate new debate by moving away from increasingly stale "crisis" debates in analysis of the fourteenth century. There is a useful bibliography of over six hundred fifty works, weighted rather heavily, it must be said, on the English side. But this is a minor complaint about an excellent and meticulously presented book.

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GERARD SIVÉRY. *Marguerite de Provence: Une reine au temps des cathédrales*. Paris: Fayard. 1987. Pp. 301. 98 fr.

Marguerite of Provence was queen of France for over sixty years and the wife of the sainted Louis IX. It is on Marguerite as wife rather than as queen that Gerard Sivéry's biography dwells. Much of this book is a history of Louis himself and his politics. Although some such background is necessary to understand Marguerite, and her familial role was crucial, the nature of Sivéry's emphasis on both has produced serious problems of interpretation.

The stock picture of Marguerite is of a frivolous spendthrift locked in rivalry with her mother-in-law early in her life. In her middle years she is alleged to have pursued Anglophile policies, often in opposition to Louis, motivated by her family ties with her sister Eleanor, Henry III's queen. In her later years Marguerite was obsessed with claims to her inheritance in Provence. Sivéry seeks to modify this picture and to replace the image of a self-willed woman with that of a submissive wife. Thus, negotiations by Marguerite with the English in the early 1260s become actions at Louis's behest, not in opposition to him but as a deliberate design to leave his hands free to arbitrate and draw his own profit. Louis and his son pursue Marguerite's claims in Provence only when it suits them. Louis never sacrificed the interests of the state to those of family, and Marguerite, a woman devoid of ambition for power, was happy to accept this. The contrast between her and her sister Eleanor is

drawn throughout, primarily to illuminate the husbands. Eleanor was the dominant, avaricious wife of the weak Henry III, Marguerite the loving and obedient spouse of the strong Louis IX. The definition of such a wifely role protects Louis's image from imputations of marital discord or uxoriousness.

Any assessment of medieval personality is suspect; this particular one rests more on speculation and nineteenth-century familial stereotypes than on evidence and deploys an antifeminist armory. Louis felt that his wife was lacking in political intelligence, that she was one-sided, emotional, intransigent, and, in any event, uninterested in power. She was a woman who could only rule in an emergency. Yet Louis had to deprive his wife of a regency that she actively sought in promises from her son. She organized the fleet and the defense of Damiette during Louis's captivity on crusade. She conducted English negotiations and ably administered and defended her dowry. Sivéry's angel of the hearth is not the appropriate familial model for a medieval queen.

To understand such women we need to establish a comparative framework of female action, of female political action, and of the presentation of that action, which cannot mean simply slotting them into the history of kings. The nature of marriage, of family relationships and property distinguished by class, of the household, of traditions of queenly action, of the discourses of chroniclers all require attention. On these topics Sivéry is at best brief and usually silent. A great knowledge of the reign of St. Louis is no substitute for these tools of women's history.

PAULINE STAFFORD
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OLIVIER GUYOTJEANNIN. *Episcopus et comes: Affirmation et déclin de la seigneurie épiscopale au nord du Royaume de France (Beauvais-Noyon, X^e-début XIII^e siècle)*. (Mémoires et documents, publiés par la Société de l'Ecole des Chartes, number 30.) Geneva: Droz. 1987. Pp. lxxv, 314.

Recent French scholarship has largely ignored the role of episcopal lordships in the political history of medieval France, and we still do not understand exactly how some prelates succeeded in creating veritable principalities (six became peerages in the thirteenth century). The only systematic presentation of this topic to date is Reinhold Kaiser's wide-ranging comparative study, *Bischofsherrschaft zwischen Königtum und Fürstenmacht* (1981). Kaiser concentrated, however, primarily on episcopal cities before the twelfth century and relied entirely on published materials. Thus, Olivier Guyotjeannin's monograph on the episcopal lordships of Beauvais and Noyon, grounded as it is on an exhaustive survey of the manuscript sources, represents a path-breaking exploration of a major neglected subject.

How did the prelates of these two second-tier sees obtain the royal rights ordinarily delegated, from the ninth century, to counts? The case of Beauvais is clear: in 1015 the bishop purchased the county from Count Eudes II of Blois from whom the bishop had held in benefice various comital rights, such as coinage, taxation, and justice. At the same time in Noyon, where royal authority apparently was never delegated to a count, the bishop, with the aid of his knights, simply expelled an overbearing royal castellan, leveled the royal tower, and assumed control of the county. Such acts culminated decades of gradual assertion of episcopal authority over a number of cities and their counties and the corresponding withdrawal of lay authority. By the mid-eleventh century, powerful episcopal lordships had emerged in Reims, Châlons-sur-Marne, Langres, and elsewhere in northern France through a variety of circumstances. Perhaps the most intriguing finding here is that the sizable contingents of knights resident in Beauvais and Noyon from the early tenth century played an important role in the formation and administration of those episcopal lordships; Guyotjeannin reminds us that the history of knights in this transitional period remains to be written.

The longest part of the book is devoted to the evolution of the two lordships through the reign of Philip Augustus. Guyotjeannin considers first the prelates and their careers, then the administration of the temporal, and finally, in an extended prosopographical study, their feudal tenants. A particularly satisfying aspect of Guyotjeannin's approach is that, while it identifies some common features (such as the late feudalization of both the Beauvaisis and the Noyonnais), it recognizes local peculiarities and resists forced generalizations. The bishops of Beauvais, for instance, acted as powerful feudal magnates, as the military adventures of Henry of France (1148–62) and Philip of Dreux (1175–1217) illustrate, whereas those of Noyon, lacking a cohesive territory and surrounded by powerful castellans, conducted themselves more discreetly. Yet both prelates became peers of the realm as Philip Augustus reimposed royal authority over them.

This original study should encourage the investigation of other episcopal lordships, for which its questionnaire and methodology will serve as a point of reference. One issue not addressed because of chronological limitations deserves closer scrutiny: the selection of the ecclesiastical peers

and their political role in the thirteenth century. While the subordination of Beauvais and Noyon to Philip Augustus in one sense marked their decline, it also opened a "second age" of episcopal lordship in which the protection and prestige of royal affiliation in many respects outweighed the insecurities of the earlier age of independence.

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CLAUDINE BILLOT. *Chartres à la fin du Moyen Age*. (Civilisation et sociétés, number 76.) Paris: Ecole des Hautes Etudes en Sciences Sociales. 1987. Pp. 360. 220 fr.

This volume is in some respects more a work of geography than of history. The author has examined an impressive array of source materials that covers virtually every aspect of human life at Chartres in the late Middle Ages. Yet these materials are too scattered to permit thorough treatment of any single aspect of medieval life as it evolved through time. The documents provide information from various years during the fourteenth and fifteenth centuries without permitting a clear sense of chronological development.

To deal with the problem presented by her sources, Claudine Billot has arranged her volume topically. The first part contains two chapters entitled "Time" and "Space." The second part has three chapters—"The Administrative Function," "The Religious Function," and "The Economic Function." A final section, on people, has chapters entitled "The Family," "Mobility," and "The Life of the Mind." Had her sources been more concentrated and less scattered over time, Billot might have given us one of those "total" histories of a region that have done so much to illuminate our understanding of the fourteenth and fifteenth centuries. Instead, she has had to be content with a portrait of a society during the late Middle Ages or, to use a more accurate metaphor, an album full of snapshots taken at different moments between 1300 and 1500.

If these two centuries had been a period of little or very gradual change, the strategy might have been valid, but, because the period was notable for major upheavals, the reader is frustrated by Billot's inability, in most cases, to document changes at Chartres in a way that permits comparison with findings for other regions in the same period. The scholarly apparatus is also disappointing. There are no notes and only occasional references in parentheses in the text or accompanying a chart. The bibliography gives an impressive list of published and unpublished sources but only a selective list of secondary works that omits some titles

that Billot surely consulted and others that she ought to have.

These deficiencies make the book rather hard going. It contains a good deal of interesting and useful information, but scholars would have found it more useful if this information had received specific documentation in notes. The principal theme to emerge is that this town's great days were behind it. Once an important religious, artistic, and intellectual center and the capital of a county, Chartres had become primarily a market town in a grain-exporting region, an important part of the trade route between Orléans and Rouen except for the period of Burgundian and English occupation (1417–32) when the town was in a war zone and cut off from its traditional economic partners.

This is a useful book in some respects, but it will not find a place among the great works of regional social history produced in France over the past generation.

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RICHARD C. TREXLER. *The Christian at Prayer: An Illustrated Prayer Manual Attributed to Peter the Chanter (d. 1197)*. (Medieval and Renaissance Texts and Studies, number 44.) Binghamton, N.Y.: Medieval and Renaissance Texts and Studies. 1987. Pp. 260. \$25.00.

Peter the Chanter (fl. 1170–97), influential scholar and theologian in the Paris schools and author of a summa on the sacraments and the more widely known *Verbum abbreviatum*, also wrote a work called *De penitentia et partibus eius*. One book of this larger work is the little-known *De oratione et speciebus illius*, which describes the seven postures for praying and is essentially an instruction manual for prayer in words and pictures. Richard C. Trexler (with frequent and gracious acknowledgment to John Baldwin's magisterial study of Peter the Chanter published in 1970) here discusses the *De penitentia* as a whole and the verbal and pictorial texts of *De oratione* in particular. Trexler's interest in devotional behavior is evident in this most interesting contribution to our understanding of the medieval view of prayer.

Trexler divides his study into three parts. In part 1, there is an analysis of the textual and illustrated contents of the book on prayer and an account of the manuscript tradition of the *De penitentia* as a whole and the relationship of the *De oratione* to the larger work. Part 2 consists of the pictures (pp. 133–64) and part 3, the edition of this previously unedited text, the *De oratione* (see pp. 171–234), based on MS. Stift Klosterneuburg

n. 572, collated with eight other extant texts of the work.

In his text, Peter the Chanter identified the various types of prayer (heart, word, and body) and considered the proper time and place of prayer. He stressed the importance of the schools as places where people learn to pray meaningfully. To this Paris master, reading and praying were closely allied. Peter defined prayer in active terms: "the individual's capturing [*captatio*] of the benevolence of God" (p. 179).

The illustrations that accompany the text, undoubtedly added later to the manuscripts, distinguish this prayer manual as a unique contribution. The text provides the background for the fifty-six illustrations of diverse postures and shapes depicting the seven modes of prayer here reproduced from eight of the extant manuscripts. The original pictures include illuminations in several colors, sepia sketches, and finished sepia drawings. Trexler emphasizes the complexity of this pictorial tradition. One must examine not only the relationship of the pictures to the texts of the individual manuscripts but also the relationship of the pictures of any one manuscript to those of the others.

We are indebted to Trexler for this comprehensive study of a little-known work by a well-known Paris master. Peter the Chanter's *De oratione* is a book that is unique in its linking of pedagogy and images.

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BRONISŁAW GEREMEK. *The Margins of Society in Late Medieval Paris*. (Past and Present Publications.) Translated by JEAN BIRRELL. New York: Cambridge University Press and Editions de la Maison des Sciences de l'Homme, Paris. 1987. Pp. xi, 319. \$44.50.

First published in Polish in 1971 and translated into French in 1976, Bronisław Geremek's classic study of marginal people has finally been translated into English, making available to students and nonspecialists a work that has been fundamental in inspiring and defining the terms of subsequent research into the lower reaches of medieval society.

To praise Geremek's careful scholarship and thoughtful analysis would be to repeat previous reviews of the earlier editions of his work. The book is based on the exhaustive study of all extant judicial records for the city of Paris, supplemented by documents from the archives of Parisian hospitals and manuscripts from the Bibliothèque Na-

tionale. The author proceeds group by group—criminals, clerks, beggars, the ill, prostitutes, and pimps—evoking through case histories the lives, the "recruitment" into marginality and criminality, and the relation to "normal" society of each group.

The boundary between the normal and the marginal worlds was, Geremek readily agrees, "uncertain and fluctuating" (p. 301). He does not give a precise definition of marginality but evokes certain common elements: a "lack of fixed abode," which was linked to a "lack of a steady income" (p. 276); "the fact of not being attached to a family" (p. 277); "non-participation in production" (p. 300); and youth (p. 286). He refuses to equate marginality and criminality (p. 3) and concludes that "the distrust which they experienced formed the cement which bound these very diverse categories together" (p. 300). Geremek is here touching on the fundamental idea that society's paranoia, the deep suspicion and fear of late medieval authorities, led to the categorization of many groups as suspect and marginal, an idea studied in depth by Jean Delumeau in his *Le peur en Occident* (1978), in which he includes heretics, Muslims, Jews, witches—and women in general. The psychological aspect of the problem is all the more important in that many of Geremek's marginal people were in fact "in close contact with the organized structures of society: the prostitutes, the lepers and the beggars," and, we might add, the clerks (p. 302). Prostitutes, for instance, were often protected by seigneurial or royal safeguard, and their rape was usually considered a crime and punished accordingly (Geremek's statement to the contrary is based on a sixteenth-century legal treatise, not on medieval practice [p. 225]). In the face of such "integration" and institutionalization, one wonders why Geremek is so reticent about emphasizing the role of criminality—a theme to which more than half of his book is nonetheless devoted—in defining marginality. Were there truly "elements who were vilified [*sur qui pèse l'infamie*], but who did not commit crimes" (p. 3)? This was true in Roman law, but, by the late Middle Ages, most undesirable behavior was clearly defined as illegal. For example, whereas the licit prostitutes and brothel keepers of the late Middle Ages were protected by the law, illicit procurers were actively prosecuted and punished.

The Cambridge University Press edition of Geremek's work is not without its drawbacks. One wonders why the text was translated from French into English rather than directly from Polish into English and why, in an edition aimed at nonspecialists, no attempt was made to translate the lengthy, difficult, and often crucially evocative quotations from François Villon and other con-

temporary literary sources. One regrets, too, the absence of any attempt to bring the bibliography up to date.

But no amount of editorial problems—or debate on the definition of marginality—can detract from the value of Geremek's work. His contribution to our thinking on the lower elements of society, inspired by both Catholic and Marxist concerns, has been incalculable. That the book is still provoking reflection and reinterpretation seventeen years after its publication is the greatest tribute to its stature.

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STEPHEN MURRAY. *Building Troyes Cathedral: The Late Gothic Campaigns*. Bloomington: Indiana University Press. 1987. Pp. xviii, 257. \$47.50.

Stephen Murray's exhaustively detailed history of the later phases of the construction of Troyes Cathedral is based chiefly on an abundance of primary written sources. Study of these fabric accounts—he calls them an “embarrassment of riches” (p. xvii)—results in a wonderful evocation of a kind of time-lapse photography, speeded up, of the rise and fall and replacement of building elements over a period of about two and one-half centuries (1294–1549).

One of the great delights of this book is the mastery of the author in organizing the often ambiguous and laconic items in the construction records into several major “campaigns,” each a “unified phase of construction embodying a coherent and unified vocabulary of forms” (p. 5). The painstaking and deceptively simple elucidation of these “campaigns” evolves as a smoothly running, sometimes year-by-year account that manages to make clear the function and hierarchical organization of the workshop, to identify several of the master masons and their special contributions, to illustrate the methods of construction and the sequence of stylistic changes, to deal with particular stylistic dilemmas such as the problem of reproducing outdated forms or inventing new ones, and, of no little significance, to record the sources of funding and to attempt to relate these varied sources to the evolving structure.

What gives the book its special force is the effort to impart some substantiality to the tedious and abstract rehearsal of the fabric account by seeking its true physical manifestation in the various structures of the cathedral itself. “Historians of Troyes,” Murray writes, “have been aware of the importance of the fabric accounts . . . since the early nineteenth century, but little attempt has

been made to correlate the evidence from the accounts with the study of the building itself” (p. 227, n. 4).

The outcome is the most complete picture in the literature on Gothic architecture of the interaction among fluctuating economic factors, the changing nature of the workshop, and a developing cathedral. The general reader, particularly the nonspecialist, who believes that the construction of a cathedral must have been, however slow, a consistently progressive venture, is in for a few surprises. It was an almost Kafkaesque venture filled with unexpected trials and setbacks that reflected not so much varying political circumstances—the impact of these contingencies is difficult, in any case, to demonstrate—as natural disasters and miscalculations on the part of the builders to an incredible degree. “We are so used to the interpretation of the medieval cathedral as the embodiment of sublime neo-Platonic Ideas,” Murray states, “that it comes as something of a relief to find that its builders were human and made errors” (p. 9).

All of this does not mean that Murray refuses to admit the existence of an aesthetic category; it means that such a category is simply “a matter of personal taste” and that discussion of it “would be like tilting at windmills” (p. 112).

For a detailed, honest, to the point, running account of what went into the construction of a great medieval edifice there is no better book than Murray's, nor is there a more noteworthy counter to the traditional stylistic approach of the standard histories of Gothic architecture. (Note, most recently, Bony, 1983.) Nevertheless, the absence of any sustained effort to present Troyes as an aesthetic, religious, or philosophical phenomenon is the equivalent of limiting a discussion of the *Divine Comedy* to syntax, rhyme, and sentence structure.

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KENNETH BAXTER WOLF. *Christian Martyrs in Muslim Spain*. (Cambridge Iberian and Latin American Studies, History and Social Theory.) New York: Cambridge University Press. 1988. Pp. xi, 147. \$34.50.

This slender but handsomely produced volume explores the motives of the priest Eulogius, the contemporary chronicler and apologist of the martyrdom movement during which the Muslim authorities of ninth-century Córdoba executed forty-eight Christians (between 850 and 859) for blaspheming against Islam. Kenneth Baxter Wolf rejects post-sixteenth-century interpretations of this phenomenon that attributed these Christians'

actions to their spirit of religious or nationalistic assertiveness or to the instigation of Eulogius himself.

Wolf seemingly sympathizes with critics within the Cordoban Christian community itself in viewing the victims' actions of defiance as unfounded and mystifying, occurring as they did among Christians whose accommodations with their Muslim masters had gained for them a measure of prosperity and tolerance. Since the actions of these so-called martyrs were not provoked by officially sanctioned persecution (Wolf, to the contrary, believes that most anti-Christian legislation was unenforced until after the onset of the martyrdom movement), he argues that these individual acts of Christian defiance as well as Eulogius's decision to record and defend them must derive from idiosyncratic and thus essentially personal motives unrelated to the general status of Christians at Córdoba. For Eulogius, the motivation came from his displeasure with the local bishop, Reccafredus, who had readily cooperated with the emir in pressuring the Christian community to halt further acts of blasphemy against Islam. By presenting these executed Christians as true martyrs, Eulogius thus intended a comparison unfavorable to the collaborationist bishop and his supporters.

The forty-eight Christians themselves are depicted by Wolf as troubled, ascetically minded individuals who might well have spent their lives secure in a monastic retreat were it not for the example of one Isaac, a former official of the emir who had presumably lost his position on account of his Christianity to an individual who abjured his religion for Islam. By thus defying the law and suffering execution, Isaac suggested to these other troubled souls an ultimate method of ascetic renunciation.

Wolf's cogent analysis of the personal side of this movement enriches our knowledge of the principal figures in the story, yet at the same time he seems to make the affair trivial by reducing it to a series of copycat suicides chronicled by a disaffected cleric. Can we forget that, whatever their individual motives, Eulogius and the Christians he memorialized were members of a Hispanic Christian community under stress? Eighty percent of them, if Thomas Glick is correct in his estimate, eventually converted to Islam. Robert I. Burns has shown that the Mudéjars of thirteenth-century Valencia, whose situation is certainly parallel despite the juxtaposition of religions, suffered degradation to their culture and status despite the absence of official persecution. Thus, the author's implication that this body of Christians was not threatened simply because direct persecution was absent does not ring true. Eulogius and the mar-

tyrs need to be placed within the wider context of their community.

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BERNARD F. REILLY. *The Kingdom of León-Castilla under King Alfonso VI, 1065–1109*. Princeton: Princeton University Press. 1988. Pp. xx, 406. \$54.50.

Bernard F. Reilly is a master of his subject, the history of medieval Spain in the period of the eleventh and twelfth centuries. Readers of this book will find a meticulously documented and closely argued narrative that begins with a description of three kingdoms—Galicia, León, and Castilla—unified under Fernando el Magno but distributed by him at his death to his three sons. Next Reilly describes the unification of the three realms under Alfonso and then proceeds to a series of twelve essays that progress both chronologically and thematically through the remaining thirty-three years of the king's reign. The book concludes with a "meditation" on the life of Alfonso VI. This political history draws mainly on the tradition of diplomatic historiography and on the author's wide reading in secondary works. The thorough research that underpins this study has yielded about two hundred twenty charters of Alfonso VI, an average of five per year.

The book's major flaw is that Reilly, in an attempt to break cleanly from previous interpretations, has created a nearly insoluble problem: a base of valid data so narrow that he must continually resort to speculation or, ironically, to acceptance of evidence from the same noncontemporaneous chronicles and epic literature that he more often rejects.

In the chapter "Court, Church, and Politics," Reilly attempts to describe the entourage of the king between 1076 and 1086, a period for which he has found some forty-five charters, of which thirty survive with lists of confirmants. On the basis of this evidence he identifies notaries; court officials, such as a majordomo and alferrez; clerics; and magnates whose names appear on royal documents. Many of his observations, such as noting a lesser presence among confirmants of Galicians from the fringe of Alfonso's realm, are not likely to incite controversy.

But Reilly repeatedly presses the documentation for conclusions that cannot be sustained from the evidence. On page 144 he states, "The resultant general curia was recognized in the practice of the reign even if it had not yet found its reflection in the language of the age." I am not convinced. Nor am I persuaded, a few pages later, to believe

that "the traveling court of Alfonso VI in the winter and spring of 1075 probably consisted all told of 226 persons, 51 carts, more than 200 head of horses, mules, and jackasses, and a small herd each of cattle and sheep at its very smallest, and may often have swollen by as much as a quarter of its size." Better evidence from neighboring Catalonia would suggest that a retinue of such numbers is excessive for the ordinary displacements of the king.

Furthermore, although Reilly on several occasions stridently dismisses the conclusions drawn from literature of Ramón Menéndez Pidal, he occasionally cites instances of documentary confirmation from the literary tradition (for example, p. 165). And Reilly does not necessarily accept all of the charter evidence at face value (for example, p. 175, n. 45).

With Reilly's study do we come measurably closer to a true understanding of this period in Spanish history than with those afforded by previously fashionable institutional and nationalistic interpretations? Or does this work present us with pseudo-objective history that reflects at least as much a late twentieth-century political science perspective as it does a true eleventh-century reality?

Can we be sure about following Reilly as he imputes peninsular strategic thinking to Alfonso: "The king of León was undoubtedly aware of al-Mutamid's repeated appeals for assistance from that quarter [Africa], but he may also have been quite properly skeptical of the ability of the Andalusian to persuade his rough Berber coreligionists to intervene in peninsular affairs. Just as reasonably, Alfonso may also have believed that the Moroccans could not successfully mount a formidable campaign at such a distance from home even if they did decide to intervene" (pp. 166–67). The point is not that Reilly is necessarily wrong but that his conclusions are based as much on modern interpretation as are those of the predecessors he criticizes.

This study is a book for specialists. It is a guide to archival documentation and a work that anyone hoeing the historical furrows of eleventh-century Castile-León will be obliged to consult.

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Futurepast: The History Company
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ADELINE RUCQUOI. *Valladolid en la edad media*. Volume 1, *Genesis de un poder*; volume 2, *El mundo abreviado (1367–1474)*. (Junta de Castilla y León, Consejería de Educación y Cultura; Colección de estudios de historia.) Valladolid: Sever-Cuesta. 1987. Pp. 390; 560.

This excellent study is a tightly controlled and strongly interpretive history that, for modern historians, complements Bartolomé Bennassar's *Valladolid* of the Golden Age and, for medievalists, stands as a beacon among studies of medieval Castilian cities and towns. The strengths of Adeline Rucquoi's *Valladolid*, which is very different from Bennassar's study, are narrative and interpretation rather than quantitative analysis, which, in any case, the surviving sources scarcely permit. The medieval documentation (public and private, published and unpublished, newly discovered or freshly presented here) is characteristically terse, incomplete, and widely dispersed in numerous secular and ecclesiastical archives. The author mines all of these records, and she surmounts their shortcomings and difficulties with imagination and restraint, drawing on theoretical conceptions of urban history developed by Paul Claval, Yves Barel, and other French scholars as well as on the suggestive research of Jean Gauthier Dalché on Castile and on the best Spanish scholarship. There is at present an explosion of research on local history in Spain, which is now struggling for interpretive freedom from the shackles of triumphalism, provincialism, and archival literalism. Rucquoi's study avoids all of these dangers and proves how much can be learned when new questions are posed and attention is paid to context.

The two volumes, which the author describes as the "vida y muerte de un sistema urbano" (vol. 2, p. 16), cover respectively the years 1265 to 1367 and 1367 to 1474. The first explores the development of an urban system in which political power, initially bestowed by royal privilege, was exercised through the *consejo*, or town council, by alternating groups of rival families, drawn from a militarily and commercially minded patriciate of caballeros. These notables governed successfully an autonomous Valladolid—in light of their own interests, of course—by defending a coalition of common purposes among the city's citizens against the often-competing interests of groups alien to the urban community, especially the monarchy, the high nobility, and the church.

The second volume analyzes the disintegration of the old system, a process that gained momentum as Valladolid was "territorialized," that is, increasingly made to bear unaccustomed burdens as a national center of royal, noble, and ecclesiastical presence and power. The city's autonomy began eroding in the early fourteenth century, and the ensuing crises (royal minorities, civil wars, famines, epidemics) often accelerated pressure for change directed from without. Valladolid was developing as a principal royal residence and seat of government as well as a favorite haunt of the kingdom's most powerful noble families. By the

middle of the fourteenth century, these "foreign" elements were able to insinuate their own divergent agendas into local government, the economy, and city life and thus gradually to destroy the city's autonomy and its productive and commercial elements led by the old patriciate. The urban system weakened dramatically with the arrival of Enrique II, continuing to decline through the reign of the Catholic kings, even as the city prospered. While this process is closely linked to the ascendancy of the Trastámara dynasty and its noble supporters, their undermining of the traditional consensus in Valladolid was decisively aided by the collaboration (*traición*) of many insiders. The old patrician families gradually abandoned their commitment to community solidarity in order to pursue the offices, titles, privileges, rents, speculative profits in real estate, and other perquisites characteristic of the landed aristocracy. Yet loyalty in some quarters to the older urban objectives goes far to explain, suggests Rucquoi, Valladolid's foremost position in the *Comunero* revolution of 1520–21.

Such a broad summary fails to do justice to the skillful expositions of evidence and judicious development of arguments that characterize these fine volumes. They also present a wealth of detail about the changing physical and social structures of medieval Valladolid and the operations of city government, politics, taxation, economic growth and decline, ecclesiastical institutions, the university, patrician and noble families, and a wide variety of other citizens, customs, and changing features of the urban scene. This is a notably rich and well-crafted work of exceptional interest to all who study medieval Castile or medieval European cities. The Spanish is clear, concise and graceful, and the work includes generous topical bibliographies, invaluable genealogical tables, and numerous well-drawn city maps.

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JEAN-CLAUDE MAIRE VIGUEUR. *Comuni e signorie in Umbria, Marche e Lazio*. (Storia degli stati Italiani dal medioevo all'unità.) Turin: Utet Libreria. 1987. Pp. viii, 293.

Any reviewer would look forward with pleasure and high expectations to a book devoted to areas of Italy that, with a few obvious exceptions, have been largely ignored. This book deals roughly with the mid-twelfth to the mid-fifteenth centuries (ca. 1150–1450). In some two hundred sixty pages, five chapters treat lords (*signori*) and peasants, communes in the consular and podestarile period, the evolution of popular regimes, the

evolution of society and the decline of communal civilization (*civiltà*), and the aristocratic recovery and the formation of signories.

A reader immediately seeks to know why Jean-Claude Maire Vigueur selected the three regions treated here, Umbria, the Marches, and Lazio. What is the nature of their unity, or why were they chosen for contrast? The author mentions their common membership in the papal states and notes that they have not been the object of sufficient modern scholarship. What he does not show is how these three areas constitute a region, geographically, economically, socially, or even politically.

The strengths and weaknesses of this book have a common cause. Based entirely on secondary works and a few published sources, it brings together and summarizes the results of numerous studies. Treatment of the issues examined is uneven and sporadic, depending greatly on the nature and quality of secondary works. The book touches on well over one hundred distinct topics. That is both its great strength and its principal shortcoming. In numerous cases the book provides convenient references for individual issues or starting points for research. Its many subsections contain excellent observations and generalizations.

Vigueur discusses major questions of medieval Italian historiography, from the nature of Italian feudalism to the debate over the flight of serfs from the city. Numerous vignettes and specific examples put flesh on the skeleton. One such is a discussion of a pact between certain lords and the communes of Spoleto and Narni, which shows the variety of advantages that lords derived from serving as hired communal cavalry. Another is an informative description of the creation in the last decade of the twelfth century of the commune of Montalboddo, in the province of Ancona in the Marches.

The reverse of the coin is that Vigueur deals too briefly with too many topics, devoting about two pages to each. Many are major and crucial issues. An approach intended to clarify results in discontinuity and fragmentation. At times one almost visualizes an underlying collection of notecards. Many sections are, in essence, summaries of one or two secondary works.

The book contains insufficient chronological development. Too often Vigueur treats centuries in a paragraph or two. To allow one example to stand for many, he devotes but a single sentence to what he views as a major contrast between feudalism and its effects in Lazio and in the Marches. Much material is not drawn together and summarized, as the book's structure demands. Nor are we provided overarching theses that relate to all or

major parts of the book. All told, however, illuminating remarks on a great variety of topics outweigh these shortcomings.

The book contains a very useful bibliography but could profit from regional maps.

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PATRICK J. GEARY. *Before France and Germany: The Creation and Transformation of the Merovingian World*. New York: Oxford University Press. 1988. Pp. xii, 259. Cloth \$29.95, paper \$11.95.

This volume by Patrick J. Geary is a brief survey of that period extending from the late antique world to the medieval. His central focus is Gaul and western Germany, the area settled by or eventually dominated by the Germanic people called the Franks. This is essentially a work of synthesis, making available to an English-speaking audience the recent work (mostly in German and to a lesser extent in French) of historians, ethnographers, and archaeologists. In the process Geary hopes to do away with some of the old myths surrounding this period, myths based (as he explains) on Greek and Roman attempts to understand the barbarian world in terms comprehensible to themselves, on Carolingian propaganda against the Merovingians, and on nineteenth- and early twentieth-century nationalist hostility between France and Germany that inevitably colored the work of French and German scholarship.

Geary does this very well. Instead of trying to pin down the critical period of shift from Roman empire to Germanic kingdom, he presents a picture of long gradual change wherein the barbarians became more Romanized and the Romans became more barbarized until eventually there was no way to distinguish one from the other. We are thus invited to give more weight to developments in the Roman provinces, especially in Gaul, from the late second and early third centuries on, than to shifts in imperial policy guided from wherever the capital of the Roman world might have been at any particular time, and we are asked to revise our opinion of the later Merovingians who may well not have been as ineffectual as Carolingian propaganda would have us believe. In addition, Geary succeeds in taking his readers through the extraordinarily complicated political history of the Merovingians, from before the time of Clovis to the triumph of the Carolingians.

Geary's book is not a monograph but a synthesis based on a wide reading of the work of many specialists, embellished by the results of his own research. It is good that the results of this scholarly

work should be made available to American students, so few of whom can today read the work of French and German scholars in the original languages. It is also good that students of history continue to recognize the importance of specialists in other fields, especially since in the period under discussion the literary sources either are entirely lacking or are the product of peoples of other cultures who did not completely understand the non-Roman world. Historians of the late Roman and early medieval periods are increasingly indebted to anthropologists and archaeologists for correction or substantiation of the written sources. But there are problems here, too, and one could wish that Geary had pointed them out. After all, the raw data collected by the modern archaeologist or ethnographer are meaningless until the material is organized and questions are asked of it—and, just as in the writing of history, the cultural background of those who do the organizing and who ask the questions will inevitably influence the answer given. I think that this is especially true in dealing with institutions that are to be found in modern nontraditional societies as well as among the non-Romanized peoples of Europe in the late antique period. The Latin and Greek writers may well have been seeing the barbarian world through eyes reflecting their own civilization, but they were certainly closer to the barbarians and understood them better than a present-day ethnographer working among primitive peoples. Therefore, although I am in complete agreement with Geary in his insistence on the strong Roman influence in the development of Germanic institutions and in the creation of the Germanic kingdoms (in this case the Frankish kingdom), I am much more hesitant to accept his description of the early Germanic family or the function of clan and feud in the creation or destruction of tribes.

But, since Geary himself has pointed out that every statement in his book needs a whole volume to support or justify it, it is not fair to criticize individual matters. Certainly undergraduates will appreciate having such a clear statement available about that period of French history so often simply skipped because it is too incomprehensible.

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MODERN EUROPE

MICHAEL R. MARRUS. *The Holocaust in History*. (The Tauber Institute for the Study of European Jewry Series, number 7.) Hanover, N.H.: University Press of New England, for Brandeis University Press, Waltham, Mass. 1987. Pp. xv, 267. \$16.50.

Michael R. Marrus's previously demonstrated virtues and interests as a scholar make him unusually well qualified to take on the difficult task of assessing the historical literature dealing with the Holocaust. The product of his labors is a great success. This book easily qualifies as the most readable, balanced, and perceptive survey of the subject of the Holocaust in history in any language.

Marrus begins by making clear his fundamental disagreement with those who insist that it is impossible for us to comprehend the Holocaust, who assert that the world of the extermination camps "defies imagination and understanding; it submits only to memory" (Elie Wiesel). Although expressing eloquently his own sympathy for the victims and his awe before the enormity of mechanized mass murder, Marrus still rejects mystification. "It may well be that on some profound level events such as the murder of European Jewry will forever elude human understanding. . . . But so it is also for much of recorded history, and for much of what we encounter in daily life" (pp. 29–30).

The subject of the massacre of Europe's Jews has only recently begun to attract the attention of historians. Even the currently reigning magnum opus, Raul Hilberg's *The Destruction of the European Jews* (first published in 1961), for all of its insights and overwhelming erudition, is less a work of history than a dogged analysis, by a political scientist, of the operations within the span of a few years of a bureaucratic "system." Similarly, Lucy Dawidowicz, in spite of the rare and invaluable talents she brought to bear in her *War Against the Jews* (1975), probably the most widely read book on the subject today, may be seen as operating on the fringes of the historical profession or, as she herself might proudly insist, outside of it.

Her book *The Holocaust and the Historians* (1981) contains a heated denunciation of certain historians' treatment of the Holocaust, and one might assume that Marrus's book constitutes a response to it. He scarcely mentions her book, however, except to note that she is "oddly disdainful of much recent historiography" (p. 202). In this regard, as in many others, Marrus's tone is remarkably irenic. To be sure, he implicitly disagrees with her contention that the historical profession is being overwhelmed by scoundrels and fools (for her, synonymous with Marxists and structuralists) who must be exposed and denounced. He does discuss how scholars have demolished the rather foolish and mischievous thesis of David Irving (neither a Marxist nor a structuralist) that Adolf Hitler was not responsible for the Holocaust, but for the most part he is a gentle if also rigorous critic.

This tone holds as well for his treatment of

Dawidowicz's *War Against the Jews*; he expresses cogent but low-key critical reservations about her stubbornly "intentionalist" and bitterly anti-German theses. His own position might be termed moderately "functionalist," in that he offers a respectful presentation of the views of those historians who have argued that the shape of the Final Solution emerged not, as the intentionalists maintain, from a coherent, longstanding design but from the dilemmas, confusions, and bureaucratic chaos of Nazi policy in regard to the Jews.

Marrus expresses critical reservations, again with almost unprecedented calm, about Hilberg's central thesis that the success of the Holocaust depended on Jewish cooperation with the Nazis, a cooperation that itself derived from a millennia-old tradition of passivity. Marrus agrees, indeed, in this case with the criticisms offered by Dawidowicz and others, who have pointed out how inadequate is Hilberg's knowledge of the inner world of Europe's Jewish communities. Marrus singles out other examples of opaque language and reasoning in Hilberg's work—problems that emerged with even more troubling dimensions in Hannah Arendt's *Eichmann in Jerusalem* (1963), the explosive theses of which were based to a large degree on her reading of Hilberg.

These are only a few examples, in this admirably comprehensive yet concise and clearly written volume, of Marrus's willingness to address the major and most nettlesome interpretive issues, while analyzing them in a style that illuminates rather than aggravates. One issue that he does not address directly and indeed may not consider particularly significant—but nevertheless impressed me in reading his book—is the fruitfulness in recent years of the dialogue between Jewish and non-Jewish historians on a subject that for long was tacitly viewed by many non-Jews as monopolized by Jews and best avoided. Scholars such as Christopher Browning, David Wyman, and Ian Kershaw, to say nothing of the impressive accomplishments of German historians, have made signal contributions to our understanding of the Holocaust, at times in ways that reflect specifically non-Jewish perspectives or sensibilities and that challenge characteristically Jewish interpretations.

The point, although extremely delicate, is worth stressing, I believe, in part because the high intellectual content and balanced tone in this volume are such that it will attract a wider audience to the subject of the Holocaust. This book will also encourage a sense of scholarly cooperation, across ethnic and religious identities, in regard to the Holocaust—and thus a more general recognition of it as a central problem of the modern world,

one in which non-Jewish, no less than Jewish, historians have a vital interest.

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R. C. VAN CAENEGEM. *Judges, Legislators, and Professors: Chapters in European Legal History*. (Goodhart Lectures, 1984–85.) New York: Cambridge University Press. 1987. Pp. x, 205. \$34.50.

R. C. Van Caenegem has made distinguished and highly original contributions to the early history of the English common law system, in particular, the origin of procedure by royal writ. This volume contains a revised text of the Goodhart lectures that he delivered at the University of Cambridge in 1984–85, and the central theme of these lectures is perhaps best caught by the title given to the third chapter of the book: "The Divergent Paths of Common and Civil Law." For Van Caenegem, like other historians of the early common law, has long been intrigued by the curious fact that the common law system or tradition exists at all; given the degree of unity of European civilization, one might expect that civilization to have given birth to only one system of legal culture, and that ought to have been the civil law system.

In *The Birth of the English Common Law*, published back in 1973, Van Caenegem sought to explain this curiosity of European cultural history. In these lectures he explores a number of the characteristics of the common law system and its history that differentiate it from the civil law—for example, the different forms taken by the professionalization of the law that made judges, in contrast to professors, the dominant figures in the common law and the absence in the modern common law system in Britain of any formal constitutional text. In discussing these characteristics, Van Caenegem devotes some attention to the common law system as it has evolved somewhat differently in America. The final chapter considers the relative merits of the two traditions in terms of eight suggested criteria for good law; these include "incorruptible and impartial judges," "comprehensible and cognoscible law," and "accessible justice" (pp. 158, 160, 162).

The discussion of these criteria is uninfluenced by any hints of modern radicalism. As was appropriate for a set of lectures delivered to a very mixed audience in Cambridge, there is much explanation of matters that are already familiar to professional scholars, and the merit of the discourse lies not so much in striking originality as in clear and elegant exposition of very general themes. There indeed exists for the common law

no comprehensive and readable book to match J. F. Merryman's excellent *The Civil Law Tradition* (1969). These lectures go some way to fill the void and can be recommended to anyone looking for an introduction to the contribution of European civilization to legal evolution.

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RAINER FREMDLING. *Technologischer Wandel und internationaler Handel im 18. und 19. Jahrhundert: Die Eisenindustrien in Grossbritannien, Belgien, Frankreich und Deutschland*. (Schriften zur Wirtschafts- und Sozialgeschichte, number 35.) Berlin: Duncker und Humbolt. 1986. DM 190.

Rainer Fremdling concerns himself with the question of technological change and international trade in the iron industries of Great Britain, Belgium, France, and Germany. Although there is little question that Great Britain was in the forefront of iron technology in the Europe of the eighteenth and nineteenth centuries, Fremdling doubts that the Continent simply engaged in a catching-up process. Yet the local iron workers of particular nations had to be instructed in the new methods of iron production that were introduced in Britain. Fremdling also emphasizes cost and price comparisons. He finds that for a long time charcoal iron was sold at a quality premium over coke-smelted iron. Britain had the most time-cost advantages, he asserts, coming, among other things, from the proximity of locations of iron and coal, from a surplus of trained workers, and from a large internal market, which allowed for economies of scale. In short, Great Britain had a technological advantage, which, in turn, resulted in an export advantage. Nevertheless, British dominance in this field evaporated by the 1860s. The availability of raw materials in Europe, the lack of difficulty in hiring British specialists, and, at times, protective tariff policies helped French, German, and Belgian iron producers become competitive.

The author has written a carefully researched study, which one expects of a *Habilitationsschrift* at the Free University of Berlin. His general method of presentation is to describe the relevant conditions in a country and to elucidate on this description by presenting what he calls case studies. For example, the chapter dealing with British technology includes a section on French import duties on British iron, a British firm's export of iron to Germany, and the economic consequences of the invention of the hot-blast method of smelting by J. B. Neilson. Fremdling uses a similar method of writing for other countries. One chapter that does

not fit into this method deals specifically with the functions of the puddler. It is interesting in this connection that the heavy and highly skilled work of the puddler was not usually the best compensated of the iron workers. Generally, the man in charge of rolling the iron was better paid. Another interesting fact that comes out of this study is that Belgium enjoyed for a period of time a preferential tariff for pig iron with the German Zollverein and with France. As one result Belgian capitalists established an iron works just across the border in France and Germany and shipped to it their own pig iron, which was then further processed.

In summary, Fremdling's book is a good study of iron technology and its relationship to foreign trade in four European countries.

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GLANMOR WILLIAMS. *The History of Wales*. Volume 3, *Recovery, Reorientation, and Reformation: Wales, c. 1415-1642*. New York: Clarendon Press of Oxford University Press or University of Wales Press, Cardiff. 1987. Pp. xvi, 528. \$84.00.

In this, the third volume of the Oxford history of Wales, Glanmor Williams examines the period between the end of the revolt of Owain Glyn Dŵr and the outbreak of the Civil War, a period that saw changes in so many aspects of Welsh life. The title sums up the themes: recovery from the crises of the fourteenth century and the effects of the Glyn Dŵr revolt, reorientation when a man of Welsh extraction became king of England and Wales was largely assimilated to the English state, and reformation that not only brought religious change but also had far-reaching implications for the survival of the native language and culture.

The book is divided into two sections that deal essentially with the fifteenth and sixteenth centuries. Both sections include chapters on government, economy, society, religion, and culture, in addition to the topics peculiar to the two centuries; neither includes any of the traditional chronological benchmarks of Welsh history to start or finish. The first section ends not with the accession of Henry VII in 1485 but with his death in 1509; similarly the second does not begin with the first Act of Union of 1536 but with the accession of Henry VIII, so that the Union legislation can be seen in its proper context.

The reader will not find startling new insights here; like the other volumes in the series, volume 3 is a work of synthesis. Indeed, since so much fundamental research remains to be done, it could be described as a progress report. This aspect of the work is particularly evident in the case of the

fifteenth century, a period on which relatively little has been published, but now, for the first time, there is a survey of the century during which Wales recovered from a traumatic revolt, played a significant part in the English dynastic struggle, and witnessed the emergence of so many landed families.

During the sixteenth century, there was continuity as well as change. Many in Wales have seen the legislation of 1536 as the beginning of a deliberate attempt to stamp out both the language and a national consciousness; Williams argues that the two acts of 1536 and 1543 did no more than give legislative sanction to changes that had already occurred. The real rulers were the gentry, the traditional leaders whose local power and influence went back to 1282 if not before. The fact that these leaders now sat in Parliament and sent their sons to English universities did not dilute their Welshness in any way.

One of Williams's particular strengths is the use he makes of the rich Welsh literary tradition. Few historians outside Wales are aware of the value of Welsh poetry as a historical source. In the poetry of the sixteenth century there is a wealth of social and religious comment, and the poems that promote the cause of Henry Tudor can shed considerable light on popular attitudes toward Richard III. The sixteenth century saw the confrontation of the traditional bardic culture and the New Learning; the work of Welsh humanists enabled the Reformation to take root by its influence on William Morgan's translation of the Bible in 1588. As Williams rightly says, "The linguistic and cultural integrity of Wales was saved by Morgan's Bible" (p. 322).

There is, of course, much more in this book. The impact of the political affairs of the kingdom on Wales in both centuries is examined; indeed, Wales is never considered in isolation. The sixteenth century saw the beginnings of industrial development, which receives due attention. Perhaps more space could have been given to social and economic aspects of Welsh history. Far more work, however, is needed in these fields, and the way ahead may lie in more detailed studies of local communities, following the example of Brian Howells's work on Pembrokeshire. Not everyone may agree with all of the author's comments or interpretations. But this is an important and valuable book; like all of Williams's work, it is clearly and elegantly written as well as based on years of research and reflection. He is the general editor of the Oxford history of Wales; here he leads from the front.

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WARREN W. WOODEN, editor. *Children's Literature of the English Renaissance*. Edited by JEANIE WATSON. Lexington: University Press of Kentucky. 1986. Pp. xxii, 181.

Before he died, Warren W. Wooden was engaged in preparation of a study "to investigate the origins of English children's literature from the introduction of printing in England through the conclusion of the seventeenth century with the aim of producing a comprehensive, book-length manuscript exploring the cross-currents of this rich but neglected area" (p. x). This posthumous compilation gathers preliminary sketches that may have the internal coherence of conference papers leading to that study but lack any larger argument or organization. There are very modest theoretical ambitions here; the essays show no trace of French sociology or of British and American social, economic, and family history. Nevertheless, the preliminary findings may have some value for the specialist in the history of childhood or in the history of children's literature (and, by extension, literacy) in England.

Wooden has left some interesting observations about the religious context of Renaissance English writing for and about children. The Catholic, Marian concept of the child as "originally innocent" is an especially intriguing find for those who ascribe the first promulgation of that "Romantic" view to Jean-Jacques Rousseau. Wooden shows how the topos of the innocent child, blessed by Christ, became the image of the simple Christian of the old tradition—contrasted by polemicists like Cardinal Pole with the unruly and childish Protestant. In like vein, Wooden shows how the Childermass sermons in late medieval England served not only doctrinal but also social disciplines. And then he shows how the most repellent texts in the English Reformation could be appropriated for childhood: the bloodthirstiness of John Foxe's *Book of Martyrs* is shown to have provided children in the seventeenth century (and later) with a comic-book blend of heroism and horror. Shrewd publishing of such materials by entrepreneurs like John Taylor, the "Water Poet," illustrate a blend of ideology and commercialism that characterized the marketing of literacy as a tool for religious indoctrination from early times into the late Victorian period in England.

Although he was an English professor, Wooden is more useful in his presentation of historical matters than in his interpretation of literary texts. For example, his treatment of John Skelton's *Philip Sparrow* seems naïve in its insistence on the childlikeness of what is actually a sophisticated, bawdy spoof. And the essays on Michael Drayton's *Nymphidia* and English Renaissance fairy poetry

validate the nonspecialist's suspicion that the explorer of forgotten Renaissance verse must traverse a boundless expanse of verbiage with little hope of finding treasure. But there is a genuine find in John Bunyan's *Country Rhymes for Children*. It would be difficult to overestimate the way in which Bunyan's writings have formed the basis for the succeeding Protestant discourse on childhood in England. In the small verses that Wooden discusses in the last chapter of this collection, we see again the sources of Bunyan's appeal—his simplicity of language and image, his directness, and the ingenuity of his interpretations of quotidian detail as they relate to matters of eternal significance.

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RICHARD LACHMANN. *From Manor to Market: Structural Change in England, 1536–1640*. Madison: University of Wisconsin Press. 1987. Pp. xiv, 166. Cloth \$35.00, paper \$17.75.

Richard Lachmann has written a very interesting short book in which he creates an illuminating model of the development of English social structure from 1250 to 1640. Using this model he attempts to explain the rapid emergence of capitalist agrarian relations between 1570 and 1640.

Lachmann argues that structural change at the national level in the relationships between three elites—the king, lay landowners (including magnates who owned over five thousand acres and large landlords who owned from one thousand to five thousand acres), and the clergy—"took temporal and causal precedence over the transition from manorial to market structures at the local level" (p. 23). Before the Reformation these groups had, roughly, equal power, and the church courts' protection of peasant manorial rights mediated local relationships between landlords and peasants. After the Reformation, the absence of a central bureaucracy meant that the king was unable to penetrate local production-based relationships. Lack of a bureaucracy, the removal of the clergy as significant actors, the inclusion by patronage of magnates in the court, and, Lachmann argues, the rupture of the magnates' local hegemony meant that large landowners were able to consolidate power locally. The strategies used by landowners to control the land led to the expropriation of peasants from their land and to the creation of an agrarian labor market.

Lachmann's argument depends on coherence and solidarity among the actors in his drama. For example, he contends that from 1250 to the Reformation the clergy and the church courts forced

new positions of tenancy into the constraints of manorial organization. Church courts supported peasants' rights in order to protect the tithes of benefice holders. When dissolution of the monasteries in the 1530s removed the clergy as mediators, a single elite confronted the peasants. "Lay landlords, in the absence of clerical regulation, were able to challenge the land rights attached to a plurality of peasant positions" (p. 31). And freeholders articulating interests contrary to the protection of copyholders' land tenure fractured peasant solidarity. Most peasants lost their land rights, and polarization began.

Lachmann presents a coherent picture of the emergence of an agrarian labor market, but his portrait is incomplete and inconclusive. In my opinion, the main weakness of the book is that many of Lachmann's contentions, including some crucial to his arguments, are analytically and empirically faulty. For example, his estimation of the power of clerical courts before the 1530s is not adequately supported in the text or in the references he cites. Similarly, some of his characterizations of sociological theory are at best misleading, as when he suggests that Max Weber articulated an "ideological determinist theory of the Protestant ethic as the instigator of capitalist practice" (p. 143).

Too often Lachmann asserts a solidarity of interest whose manifestation is debatable. Large landowners, he argues, were unified in wanting control over the land in opposition to both the peasantry and the court. He therefore characterizes post-Reformation social structure in terms of a simple court-country model. But he underestimates the court's influence in the constitution of local elites, the gentry's national role, and the influence of local magnates. He ignores structural constraints inherent in a capitalist economy as well as the differences in the structure of agrarian relationships between fielden and forest-fens, and he too often forgets the importance of noneconomic factors in constituting interests. He also ignores the consequences of conflict between a patrimonial political structure and an emerging capitalist (not merely market) economy, and thus he ignores the early Stuarts' actual policies, overestimates the early opposition to ship money, and sees the English revolution as a court-country conflict, while missing the fissures that split the gentry after 1641, fissures explicable by the emergence of capitalist relationships within the context of a patrimonial state. On occasion Lachmann's argument is further weakened by apparent contradictions. For example, manorial courts are both the landowners' vehicle for controlling the peasants (pp. 25–27, 48, 50–52, 59) and the peasants'

vehicle for articulating their interests (pp. 61, 103–04, 111–12, 117).

Even given these weaknesses, Lachmann has provided an argument that integrates national and local concerns into a provocative explanation of the transformation of English feudalism into a market economy. If his model sometimes appears too narrow, it is for that reason all the more provocative.

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BRIAN P. LEVACK. *The Formation of the British State: England, Scotland, and the Union, 1603–1707*. New York: Clarendon Press of Oxford University Press. 1987. Pp. viii, 260. \$49.95.

The dynastic circumstance of a Scottish king's succession to the throne of his vastly more powerful neighbor in 1603 gave rise to the century-long political problem that is the subject of this book. Historians used to regard the solution of 1707 as both beneficial and foreordained: no more, since William Ferguson's slashing attack on both opinions in 1977 (*Scotland's Relations with England: A Survey to 1707*, reviewed in the *AHR* 83 [1978], p. 446). Brian P. Levack wisely eschews this controversy; instead, he adopts a topical approach, analyzing the political, legal, religious, and economic aspects of the problem as they altered from one proposed solution to another between 1603 and 1707.

What was created in 1603 was a regal union, common enough in early modern Europe: two separate realms owing allegiance to the same crowned head. What was put in place in 1707, and not much altered since, was what Levack accurately describes in his title: a British state, an incorporating union, a single economic unit with a common law of succession, a combined legislature, a single treasury, and separate legal and ecclesiastical structures, with the legal structure subject to change by the combined legislature, the ecclesiastical structure not. This was far from the intention of James VI and I, who aimed at the creation of a genuine British nation. He pursued this goal even after his proposals for a partial union were blocked in the English Parliament, because he knew very well that without the creation of such a nation the union would ultimately mean English domination over Scotland. So it has turned out, in part because none of James's successors, non-Scots to a man (and woman), had any interest in providing genuine equality.

English opposition to James's plans stemmed from dread of the economic consequences of admitting the Scots to compete on equal terms

with the English and fear that union would expand the royal prerogative and create a new kingdom, thereby destroying the English constitution. Legal and religious differences caused far less concern, although the Scottish jurist Sir Thomas Craig, an ardent unionist, employed a self-defeating argument in advocating legal union on the basis of civil law. As the century wore on, the situation became precisely reversed. The churches grew further apart until, after the final establishment of Presbyterianism in Scotland in 1690, there was no basis for religious union at all—and, with the English Toleration Act of 1689 and the dawn of the Enlightenment, much less perceived need for it. Seventeenth-century Scots legal writing gave a coherence to Scots law by the 1690s that it had not had in 1603 and stressed the differences from English law rather than the similarities. On the other hand, the English Parliament's position of power in the state by 1700 allayed English fears of political and economic union provided that its terms eliminated and incorporated the Scots Parliament, because the Scots would always be a small minority in the combined body, which could legislate on political and economic issues and modify Scots law as necessary. So James's campaign speech of 1607 to the English Parliament became a fulfilled prophecy a hundred years later: "You are to be the husband, they the wife; you conquerors, they as conquered" (p. 27), an outcome that would not have pleased him.

Levack has threaded his way through much material with great skill, accuracy, and even-handedness. The topical approach is original and works well; the chapters on the law and on James's efforts to create a British nation are particularly valuable. Levack concludes that the two kingdoms had less in common in 1707 than they did in 1603, a defensible but certainly debatable assessment. This is a brief book, which necessarily eschews many of the complications, but it is the best introduction to the subject now available. All serious students of the problem should read it.

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ANDREW M. COLEBY. *Central Government and the Localities: Hampshire, 1649–1689*. (Cambridge Studies in Early Modern British History.) New York: Cambridge University Press. 1987. Pp. xviii, 266. \$44.50.

As its title indicates, Andrew M. Coleby's book is an analysis of the relations between central and local government in Hampshire from the Rump to the Convention Parliament. Coleby's strategy is comparison of his findings about Hampshire to

current assumptions about the relations between central and local government in each of three periods: the Interregnum, Restoration and Pension Parliament, and the years from the Exclusion Crisis to the Glorious Revolution. Coleby argues that there was less to differentiate the relations between local and central government in these periods than has been assumed. There was considerable continuity in the concerns of the central government in the localities throughout these four decades. Likewise, the extent to which central government actually exerted control over local government in each period was quite similar: the two regimes usually depicted as centralizing—those of the Interregnum and of James II—did not succeed in imposing their will on the localities, while the central government of the first eighteen years of Charles II's rule was more effective than has been realized.

Coleby supports his argument with examination of subjects vital to understanding both his topic and the era—with examination of office-holding in county, borough, and local military installations, of the collection of national taxes, of the prosecution of recusants, and of the militia and defense. Obviously, Coleby has illuminated subjects that have hitherto been unexplored.

So, it is unfortunate that his analytic strategy impedes his presentation. It might be very appropriate to structure a work around the comparison of findings about one locality to current assumptions about the relation between central and local government if there were a highly developed set of arguments in the literature about this relationship. There is not. True, there are studies of local government during the Interregnum that can be used for this purpose, but there are no such studies for Coleby's other two periods—1660 to 1678 and 1679 to 1688. Consequently, Coleby cannot mount a powerful revisionist argument, for much of his book deals with issues and an era that have not generated much analysis.

Obviously, Coleby's book is therefore valuable because it deals with a new theme, new subjects, and a relatively unexplored era. Unfortunately, the book's structure inhibits full development of the potential inherent in these novelties. Because Coleby proceeds by comparing his findings about each subject in each of the three periods with assumptions about the relevant period, it is difficult to trace subjects from one period to another. Similarly, the book's strategy obscures development of its theme, for the strategy allows creation of a structure for argument that does not demand delineation and characterization of the relationship of central to local government. Reflection on the nature of this relationship might have resulted in an argument more sophisticated than that de-

pending simply on the concept of centralization (a concept whose definition the book's strategy also evades). Coleby begins by noting Stephen Roberts's observation that, in this era, central and local government were not regarded as clearly separate entities. Perhaps not. But Coleby's evidence indicates that, between the Interregnum and the Glorious Revolution, these entities were, in practice, becoming more distinct. Rarely after 1660 did the central government supervise the implementation of social policy in the localities. Only marginally, by 1688, was the collection of national taxes such as the excise dependent on action by the local authorities. Perhaps the concept of structural differentiation may prove more useful than centralization for analysis of change in England's government. Clearly, Coleby has laid foundations for a new debate about seventeenth-century government—a debate about its administrative structure. It will be to his credit if his is not the last book on this subject.

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CHARLES CARLTON. *Archbishop William Laud*. New York: Routledge and Kegan Paul. 1987. Pp. x, 272. \$65.00.

Archbishop William Laud contributed to the polarization of seventeenth-century religion and politics and since his death has figured prominently in historians' explanations for the breakdown of consensus and the coming of civil war. To the Whig Thomas Babington Macaulay, he was a "ridiculous little bigot." Hugh Trevor-Roper's unrelentingly secular *Archbishop Laud* (1940) portrayed him as a practical administrator. Most of Laud's sympathetic biographers have been clerics. Thus, a sympathetic yet nonsectarian biography may be too much to hope for, since political clericalism was what Laud was all about. But in recent years new questions and new perspectives have made room for a fresh look at the archbishop. In his biography, Charles Carlton attempts to provide "a new portrait of him—perhaps of a less significant, although far more human man" (p. 2).

Well organized and engagingly written, Carlton's study is based on comprehensive research in printed and manuscript sources. No hagiography, it repeatedly depreciates the stature of its subject, "a short, red-faced little man" (p. 72). This portrait is not only physical but also psychological. Occasionally invoking Sigmund Freud, Carlton concludes that Laud's dreams were evidence of "neurosis" and "paranoia" (pp. 144, 153, 112). Laud's diary reveals its author's fantasies clearly

enough, though it will be hard to prove or disprove what effect those fantasies may have had on the archbishop's behavior or views on theology and politics. The evidence is only circumstantial for Carlton's assertion that Laud and other bishops found comfort in the theory of divine-right episcopacy because they felt insecure about their social origins (p. 195).

Some of the diary entries will admit of explanations other than those presented. Laud recorded a dream in which his patron, the duke of Buckingham, came into his bed (p. 152). Carlton concludes that "perhaps there was a latent sexual attraction between them" (p. 73). In his *Charles I: The Personal Monarch* (1983), Carlton reached similar conclusions about that king's relations with Buckingham. Such claims say at least as much about our own age as they do about Laud's, and Carlton himself warns in another context that "posterity has attributed its own goals to seventeenth-century Englishmen" (p. 205). On this subject, Laud deserves the last word: "I am not moved with dreams: yet I thought fit to remember this" (p. 185).

Laud's religion looms large in the recent debate over English Arminianism. Was the archbishop essentially a latitudinarian and an ecumenist or a full-fledged theological Arminian? Carlton considers that Laud and his colleagues "may be defined in ideological terms as Arminians" (p. 27). Ideology—in the sense of a system of theories constituting a social and political program—is a twentieth-century concept that suits Carlton's view that "Laud preferred to confine his theology to political matters" (p. 12).

In the final chapters, Carlton provides a sympathetic account of the archbishop's imprisonment, trial, and execution. He exposes the hollowness of the treason charges that Parliament brought against Laud, who, he allows, was "the perfect hate-figure, the ideal scapegoat upon whom the commonwealth's ills could be hung" (p. 208). As Carlton eloquently demonstrates, Laud as an administrator effectively deployed a small coterie of colleagues but failed to build a constituency for his program in church and state or to communicate his policies to the nation at large.

THOMAS A. MASON
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HILARY MARLAND. *Medicine and Society in Wakefield and Huddersfield, 1780–1870*. (Cambridge History of Medicine.) New York: Cambridge University Press. 1987. Pp. xxiii, 503. \$59.50.

There are many historical generalizations about the changes in medical practice during the Indus-

trial Revolution, generalizations on the division between physician and surgeon, the separation of hospitals from general practice, the prestige and influence of the medical profession, and the growth of both charitable and rate-supported systems of treatment. Hilary Marland scrutinizes these and other assumptions under a microscope focused in turn on two contrasting Yorkshire towns: Wakefield, a small, traditional market center, and Huddersfield, a "frontier town" of the Industrial Revolution.

Medical practice developed in response to the needs of the local population and varied in relation to the ability to pay. Marland describes the medical systems supported by the two towns, stressing the ability of laymen to control the activities of medical men, from the admissions policies of the new charity hospitals to the power exerted by the Poor Law guardians and numerous friendly societies. Even the poorer members of society exercised some choice over whether to apply for poor relief or resort to the cheap and less humiliating services of the local druggists, herbalists, and itinerant quacks. At all levels, the physician was subject to the wishes of clients and paymasters.

The early chapters conscientiously relate the economic and political development of the two towns. The careful argument and lively detail of the succeeding chapters illuminate the wide varieties of medical practice and the close relationships between practitioners and the communities that supported them. The historical sources are luxuriant, and Marland has made excellent use of them. One suggestive argument is that the medical services of the New Poor Law, while ending the parishes' frequent use of unqualified practitioners, nevertheless reduced the quality of medical services for the poor. The old parishes were more willing to pay for relatively costly treatment than were the new and economical guardians. Like Irving Loudon, Marland believes that the medical services of the old Poor Law have been dismissed as inadequate on little evidence. The quacks, however, continued to flourish after 1834, in spite of the rising standards of the qualified practitioners. Alternative remedies were not only cheaper but sometimes less damaging than orthodox treatment, and they offered psychological consolations that still make them attractive to many.

The comparison between the two towns is clearly made in the earlier chapters but is less evident by the end of the book. Marland explains most interestingly the intricate relations between medicine and the community; there are sharp comments about the readiness of employers to subscribe to hospitals that increasingly treated the victims of accidents in the unregulated factories. She also examines the role of elite groups in

running medical charities and the way in which the medical profession fitted into the towns' religious and political structures, which were independent and contumacious. Medicine in its social rather than its curative role is the theme, well sustained in a scholarly fashion. An advantage of the microscopic approach is that there is time to observe individual actors, medical and lay, respectable and disreputable. Since this is Yorkshire, there is a steady procession of Holdsworths, Ramsdens, and Crowthers, all opinionated and some very eccentric.

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ANGUS HAWKINS. *Parliament, Party, and the Art of Politics in Britain, 1855-59*. Stanford: Stanford University Press. 1987. Pp. xiii, 415. \$38.50.

The 1850s have not been a period much favored by political historians of Britain. Nor is it difficult to understand why. On the face of it, all appears a welter of chaos and confusion. The former Conservative party was divided, the Peelites loyal to the memory of their dead leader. The Liberals were not in much better shape, the Whig leadership squabbling among themselves, the Liberal rank and file in disarray, and both fearing the Radicals. Had it been possible to decide on something to be Liberal about, things might have been better, but it was not. Lord John Russell plumped for parliamentary reform, which he pursued fruitlessly and with a persistence usually considered perverse. At least he wanted to do something. Lord Derby, the leader of the Conservative party, apparently wanted to do nothing, least of all be prime minister. The dominant figure for most of the period seemed to be the jaunty and pugnacious Lord Palmerston, always ready to protect British interests abroad. He appeared little interested in reform at home, but this seemed not to be essential. Prime minister from 1855 to 1858, he won a resounding vote of confidence at the polls in 1857. A misjudgment over the Orsini affair, which seemed to sacrifice British national pride to the French, forced him to give way to a minority Conservative government in 1858 and 1859. That government's most important measure was a parliamentary reform bill that failed. Finally, in a meeting at Willis's Rooms on June 6, 1859, Whigs, Liberals, Peelites, and Radicals at last, seemingly almost miraculously, agreed on a reconciliation. Politics were once again on their normal two-party course.

Though admittedly oversimplified, this account is not an unfair version of the prevailing view of parliamentary politics in the 1850s. It is a view that

Angus Hawkins attacks in almost every important detail, with a staggering amount of evidence triumphantly marshaled. The book is not easy reading and cannot be with the density of evidence adduced, but it is essential for every student of nineteenth-century politics.

Hawkins concentrates his attention on the period 1855 to 1859, demonstrating how what preceded was a vital precursor of the party stability of the 1860s and 1870s. The confusion of parties in the 1860s, he argues, was no sign of an absence of devotion to the principle, which remained as strong as ever. The problem on the non-Conservative side was that there were too many leaders advancing too many programs. The result was a total lack of cohesiveness on domestic matters, and this is what gave Palmerston his chance. Divided on the niceties of parliamentary reform and conflicting nostrums of finance, those of Liberal leanings were mostly able to rally round an essentially moderate and traditional Palmerstonian foreign policy. It was this that gave Palmerston his appearance of easy dominance. But appearances were deceiving. Whenever a serious domestic issue arose, Palmerston was in difficulty, and the fragile basis of his support became apparent.

One such issue was parliamentary reform, and here again Hawkins departs from conventional wisdom in judging Russell a shrewd tactician in choosing the issue. He points out that early in 1857 Russell was able to lead 165 members of Parliament, normally government supporters, into the division lobby in support of a reform motion, leaving a pitiful 37 front benchers to oppose (p. 57). Again he shows that the general election of 1857, traditionally heralded as a triumphant vindication of Palmerston and his policies, was nothing of the sort. The principal effect of the election was an important accession of reforming strength in the House of Commons (p. 66). Russell's association with the reform question almost made him premier in 1859, and it put him in a position to ensure a significant Radical influence in the new Liberal party.

Hawkins is perhaps at his most original in his treatment of Lord Derby. Far from being the languid aristocrat disdainful of office usually portrayed, he was, as Hawkins shows him, a politician both remarkably shrewd and highly ambitious. Out of office, he pursued a policy that Hawkins accurately describes as "masterly inactivity," committing the party to nothing that would either divide it against itself or unite others against it, at the same time giving its opponents full scope to display their own differences. In office, by contrast, Derby strove to put forward moderate and constructive measures that would build faith in the country. The Reform Bill of 1859 (of which Hawk-

ins shows Derby to have been fully in charge) is an excellent example. If Russell assured the infusion of a powerful radical and reforming element in the new Liberal party, it is Derby who deserves most of the credit for making the Conservatives a credible alternative.

A brief review cannot do justice to this splendid book. Hawkins has put forward a major reinterpretation of the 1850s. His work will richly deserve all of the attention it will receive.

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GERARD M. KOOT. *English Historical Economics, 1870-1926: The Rise of Economic History and Neomercantilism*. (Historical Perspectives on Modern Economics.) New York: Cambridge University Press. 1987. Pp. viii, 277. \$39.50.

Too little is known about the series of intellectual transitions in later nineteenth-century Britain that eroded the legitimacy of both classical economics and utilitarian social theory. Part of a new Cambridge series on historical approaches to modern economics edited by Craufurd Goodwin, Gerard M. Koot's book is much indebted to the sociological studies of A. W. Coats and others on late nineteenth-century economic thought and parallels recent research by Alon Kadish and John Maloney. Koot is concerned primarily with the intellectual rather than the institutional evolution of the teaching of economics, with the emergence of a historical heterodoxy at Oxbridge and the London School of Economics, and with the writings of W. J. Ashley, H. S. Foxwell, William Cunningham, J. S. Nicholson, W. A. S. Hewins, and Sidney and Beatrice Webb.

Except for the Webbs, these are not household names today. Their collective economic views were in fact greatly overshadowed by the neoclassicism of W. Sh. Jevons and Alfred Marshall. But the historical economists helped shift economic thought from a deductive to a historical, inductive basis and argued for the derivation of economic theory and applied economics from economic history. Moreover, they had considerable influence on national and imperial social policy and theory. Their stress on a more evolutionary, historical, and collectivist interpretation of social development met with considerable favor, Koot persuades us, in a generation grown tired of *Manchestertum*. Both reacting to increasing foreign competition, the growth of business crises, and the deepening poverty of the 1880s and seeking a compromise system between socialism and unregulated capitalism, the "neomercantilists," as Koot terms them (the term remains dauntingly complex and some-

what misleading), thus assisted in the transition from liberal to neoliberal, welfare-oriented political thought, with its emphasis on "positive liberty," and Keynesianism.

Koot dates the shift toward neomercantilism from contradictions in John Stuart Mill's social and economic thought as well as in other challenges to popular Ricardian orthodoxy beginning in the 1870s. The historical economists were also indebted to the Positivists and a variety of late mercantilist writers (such as Sir James Steuart), attracted at times to the protectionism of Friedrich List, Matthew Carey, and others, and were practically concerned with the pressing woes of Ireland in particular. Added to these concerns was the impetus created by statistics (Charles Booth showed that even the socialists underestimated the extent of poverty in London) and the development of economic history as a separate discipline, which owed much to Thorold Rogers, who saw economics as scarcely removed from practical politics. The universities contributed a variety of historically minded economists. The best known today probably remain Arnold Toynbee and W. J. Ashley, both of whom tended toward socialism, William Cunningham, who proposed a revival of something like Tudor economic management, and, of course, the Webbs.

Koot's work thus has a wider appeal than the history of economic ideas, and, by demonstrating the intimate relations between economic thought and social theory and policy, he offers a new context for understanding the work of Michael Freeden and others on new liberalism as well as an older literature on late nineteenth-century socialism. There are a few areas in which the text could have been improved. More should have been written about John Ruskin's influence in the 1870s and later. Further analysis of the types of socialism the neomercantilists embraced and also sought to avoid would have been helpful. Nonetheless, this well-written book is a highly useful addition to our understanding of late Victorian social theory and of the relations between history and economics in the period in particular.

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CECIL DEGROTTE EBY. *The Road to Armageddon: The Martial Spirit in English Popular Literature, 1870–1914*. Durham, N.C.: Duke University Press. 1988. Pp. 280. \$27.50.

Cecil Degrotte Eby's book falls somewhere between literary and cultural history. Although his main focus is on "popular literature," he provides

chapters about the Boy Scouts and about the public school ethos of "playing the game." He surveys a wide range of materials and synthesizes much previous scholarship. He writes well and is an amusing, perceptive literary critic.

At the same time, there are various inaccuracies. The British casualty figures Eby offers are for 1918, not 1914 (pp. 251–52), and G. A. Henty did not worry about "polluting the English gene" (p. 6), because "gene" did not enter the English language until after 1900. Moreover, although Eby acknowledges his debts, it is not apparent that he adds much to previous work. "Invasion scare" novels are more thoroughly treated in I. F. Clarke's *Voices Prophesying War*. For the Boy Scouts, Eby depends on John Springhall and, for public schools, on Jonathan Gathorne-Hardy and J. A. Mangan. As a literary critic, Eby is at his most original with minor writers—J. M. Barrie, Arthur Conan Doyle, and Rupert Brooke.

Apart from these reservations, the book is a skillful, often-entertaining synthesis. The story Eby tells, moreover, is an important one, which perhaps cannot be too often told. And the questions he raises cannot be easily answered. Just what was the relationship between popular writing, public opinion, and World War I? Was culture in this instance a main cause of war or more a reflection of material forces that would have led to war anyway? Eby is fully aware that the martial spirit in Britain was no more virtuous than it was in Germany—indeed, it was just as virulent. In this regard, his book bears comparison with Peter Firchow's *The Death of the German Cousin* (1986). Eby paints the development of martial spirit in comparatively broad strokes, while Firchow concentrates on the ironic transmogrification of British racist ideology from the rampant Anglo-Saxonism and Teutonism of most of the nineteenth century to the abhorrence of "the Hun" equally rampant in pre-World War I writing.

The story both Eby and Firchow tell may be less important for historians than for literary humanists. This story, of course, shows literary culture not as preserving Western civilization from anarchy (Matthew Arnold's hope) but as promoting it. Eby's category of popular literature suggests a way of salvaging the Arnoldian concept of culture: it has often been claimed that popular or mass culture promotes anarchy, while high culture (administered in proper educational doses) cures it. But Eby's treatments of such important writers as H. G. Wells, Rudyard Kipling, and Henry James complicate that gratifying division of cultural labor, and Firchow's list of prominent literary figures—George Bernard Shaw, D. H. Lawrence, T. S. Eliot, Joseph Conrad, E. M. Forster—who contributed to British Germanophobia is even

longer. Eby and Firchow might have turned to theories of ideology and social reproduction, which would have connected their studies to sociological and historical accounts of the origins of World War I. But, in any event, both Eby and Firchow offer important reminders that literary culture is not immune from the plagues of racism, jingoism, and warmongering and that poets and novelists, too, may be among the causes of international violence and holocausts.

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STANLEY WEINTRAUB. *Victoria: An Intimate Biography*. New York: Truman Talley or E. P. Dutton, New York. 1987. Pp. xiii, 700. \$26.95.

With so many biographies already written on Queen Victoria, most notably the acclaimed 1964 study by Elizabeth Longford, why now another biography, other than to commemorate the 1987 sesquicentennial of Victoria's accession? Stanley Weintraub, author of biographies of other Victorians such as the Rossettis, James Whistler, and George Bernard Shaw, justifies his biography of Victoria on the basis of new sources that have surfaced in recent years, including diaries and letters of associates of Victoria and her *album consolatum*, a collection of extracts from inspirational writings that she compiled as comfort in her mourning for Albert. Weintraub also claims to be able, in these freer times, to discuss sexual matters more frankly than did his predecessors, thus promising, as his subtitle states, an "intimate biography."

Weintraub's biography is an important addition to Victorian studies but not because of the new material, which in fact is minimal. Nor does his frank discussion of sex offer much more than the speculation of when Victoria began her first menstruation and the now-familiar suggestion that, although rigidly censorious of moral deviance, Victoria was not sexually repressed. She apparently enjoyed her sexual relationship with Albert and took pleasure in collecting paintings of nudes.

The value of the book is rather in Weintraub's convincing reinterpretation of Victoria's personality. Seeing through the mystique of Victoria as a conscientious, caring queen, Weintraub portrays her as callous, selfish, stubborn, and generally unpleasant, a view with which most of her associates would surely have concurred. Rather than remaining accessible during parliamentary and international crises, she self-indulgently maintained her inflexible holiday schedule, often at great inconvenience to her ministers. Exemplifying the cult of domesticity with her large family,

she was actually an insensitive mother who did not like children, including her own. Rarely seeing her children when they were little, she manipulated and often belittled them as adults. She even tormented her beloved Albert in his last years with vicious irrational accusations.

Weintraub offers an intriguing explanation for Victoria's obsessive mourning after Albert's death, grieving that caused her to abrogate for many years even the minimal responsibilities of a ceremonial monarch. With Victoria incapacitated during her marriage by many pregnancies, emotional depression, and political ineptitude, Albert was, in Weintraub's (and Benjamin Disraeli's) view, the true sovereign of England. After Albert died, Victoria felt inadequate to fulfill the royal duties. Her long mourning had the practical advantage, therefore, of relieving her of responsibility. Weintraub's descriptions of her frustrated ministers' endless efforts to get her to perform even the simplest symbolic acts make one sympathetic to the republican movement that was especially vociferous in the 1860s.

When Victoria in the 1870s emerged from seclusion, she distressed her ministers for the opposite reason in that, taking an active role, she threatened to overstep the limits of a constitutional, by now largely ceremonial, monarch. Presenting new insight into her relationship with Disraeli, Weintraub focuses not so much on how Disraeli manipulated Victoria through flattery but on how she took the lead in trying to shape British foreign policy. Seeing herself as a Boadicea, the jingoistic Victoria tried to push a reluctant Disraeli to take a more warlike stance on the Eastern Question. Many of Disraeli's Russophobic policies, Weintraub argues, were in fact Victoria's.

What is missing from this lengthy biography is any serious analysis of the changing constitutional role of the monarch in nineteenth-century Britain. Weintraub gives a thorough accounting of what Victoria did as queen, but he does not attempt to explain why, other than to offer the obligatory quote from Walter Bagehot and other commonplace observations. It is not clear from this book what the real powers of the monarch were at her accession nor how strictly ceremonial the role was by the end of her reign. Perhaps Weintraub did not attempt explanation because even Victoria's ministers probably could not make sense of a system that required them to make long journeys to Scotland and elsewhere to consult a person whose authority was at best ambiguous. Skirting complex questions, Weintraub does nevertheless succeed in offering a more accurate if less attractive portrait of Victoria. Without footnotes yet with a thorough discussion of sources for each chapter, this well-researched, readable biography is a con-

tribution to both popular historical literature and Victorian scholarship.

NANCY FIX ANDERSON
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G. I. T. MACHIN. *Politics and the Churches in Great Britain, 1869 to 1921*. New York: Clarendon Press of Oxford University Press. 1987. Pp. x, 376. \$72.00.

In the late nineteenth century, argument about religion played a central role in British public life. Parliamentary debate raged over issues that are now obscure, and each religious controversy produced a different set of parliamentary alliances, some bizarre, which caused much confusion at the time. G. I. T. Machin has performed a valuable service by taking a chaotic mass of information about parliamentary action on religious matters and turning it into an intelligible and interesting story.

He begins with the disestablishment of the Protestant Church of Ireland in 1869 and then discusses the bitter controversies between Nonconformists and Anglicans over a variety of issues, including English disestablishment and the extension of nondenominational elementary education. Equally important to successive parliaments was the spread of Anglo-Catholic ritualism within the Church of England, accompanied by public rioting. The queen was only the most prominent of those pressing for prompt legislative action to contain the spread of Anglo-Catholicism. Less colorful but also important, the extremely complex legislation governing the Church of Scotland paved the way for the reunion of warring Presbyterian denominations. Machin describes these controversies with an air of confidence that comes from years of research, and he provides a clear and useful narrative of religion in politics in the nineteenth and early twentieth centuries.

This book is meant, however, to be more than description; it is meant to explain the rise and fall of successive religious issues in Parliament. Although excellent as a narrative, it is much better at describing important changes than at explaining them. By the early twentieth century, Nonconformists had lost interest in English disestablishment, only to take up the education question with great passion. By the end of the First World War, they had lost interest in education and, apparently, in politics as well. The surprising strength of Scottish support for disestablishment in the 1870s disappeared by 1900, when dissenting Presbyterians had taken up the internal ecclesiastical ques-

tion of church reunion. By 1921, when the Church of Wales had been disestablished and the Church of England granted a substantial degree of self-government, the British public and Parliament had become indifferent to most religious issues.

Although Machin's narrative suggests a number of new and interesting explanations for each of these changes, his explicitly explanatory passages usually leave those suggestions unexplored. He attributes the decline of interest in religion to the decline of church membership, which amounts to the assertion that religion became less important because people became less interested in religion. The decline of religion is neither an inevitable historical evolution nor the result of some hidden hand at work in British history, dulling public interest in religion. It can be analyzed and explained, and Machin's own narrative suggests some ways that politics might have contributed to the decline of religion.

It is easy enough to see how the average churchgoer might be frustrated, and the non-churchgoing outsider bemused, by the strange political alliances produced on any given religious issue. In a general election it was not uncommon to find English Nonconformists, English Anglo-Catholic ritualists, and pro-Home Rule Roman Catholics on one side and Anglican Evangelicals, native English Roman Catholics, and ultra-Protestant antiritualists of all sects on the other.

To make things worse, frustration with parliamentary inaction led antiritualist Protestants to adopt extreme tactics, such as leaping onto the altar of St. Paul's Cathedral shouting "Protestants to the rescue." Some ritualist clergymen preferred jail to obeying church law governing the way they should face during a worship service, and extreme Nonconformists had their property sold at public auction rather than pay taxes in support of religious schools. The combination of strange parliamentary alliances and extraparlimentary extremism did nothing to improve the image of religion.

At the same time, the very success of Parliament in strengthening lay participation and self-government in the Churches of Scotland and England left the ecclesiastical world more isolated from the people. Religious debate occurred within church bodies rather than in Parliament. And the successful establishment of nondenominational state religious education removed from the churches one of their most important functions, the religious training of children.

By treating the decline of religion as an external force acting on the phenomenon of parliamentary debate, Machin missed a chance to consider the extent to which both parliamentary conflict and

legislative action contributed to the decline of religion.

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University of Iowa

HOWARD NEWBY. *Country Life: A Social History of Rural England*. London: Weidenfeld and Nicolson. 1987. Pp. vi, 250. £14.95.

Howard Newby has produced a masterly survey of the last two centuries to complement his authoritative studies of the rural sociology of modern England. The book provides a comprehensive, well-written introduction to the field for advanced undergraduates and nonspecialists.

Newby follows four main themes. Fundamental to his story is the commercialization of agricultural production from the first agricultural revolution to its postwar transformation by mechanization and agribusiness. He asks why British farmers did not adapt to the great depression of the late nineteenth century as well as the Danish and Dutch farmers who, ironically, built their fortunes on the "English breakfast" of bacon and eggs. His answer is entrepreneurial conservatism and the social cachet of command over labor in cereal farming. Newby's second theme is the class polarization of rural society from the classical three-tiered capitalist sandwich of landlord, tenant, and laborer to its twentieth-century replacement by farming owner-occupiers and farm workers. Landowners and farmers are aptly captured in telling contemporary depictions as well as in succinct analyses of their economic cooperation in investment and improvement. The benighted farm workers emerged only fitfully under the leadership of Captain Swing and Joseph Arch. More routinely, they stuck to that "dark village," the opaque rural working-class subculture centered in the alehouse and the Primitive Methodist chapel. Newby provides a full and critical explanation of their mostly unsuccessful struggles against poverty, social stigma, and political impotence. The great majority of farm workers in the last century voted with their feet and left the land. In the pastoral districts of the north and west, it was the land question that divided tenant farmers from their landlords in milder versions of the bitter semicolonial conflicts in late nineteenth-century Wales, Scotland, and Ireland. Newby's third theme is government policy together with the impact of war. Under free trade, agriculture floundered in a depression from the 1870s to 1939, broken only temporarily by the boom produced by World War I. Only after World War II did government make a permanent commitment

to price supports and planning that transformed agriculture. Newby's fourth theme is the erosion of rural distinctiveness. Rural England has been absorbed into the mass consumer society with its welfare services, although the farm worker has remained remote and dependent. In the automobile age, formerly urban middle-class emigrants have poured into the countryside seeking to purchase and consume a bucolic myth of the picturesque village. Concomitant environmentalist campaigns have tried to preserve the countryside, sometimes, ironically, "for the nation" but "from the public," and sometimes even from the farmers. More populist echoes of historic conflicts over enclosures resounded in the Kinder Scout ruckus of 1932, when hikers staged a mass trespass on the duke of Devonshire's grouse moors in the Peak District.

The main interpretive flaws in Newby's excellent survey are his supposition that food riots involved the agrarian poor and his account of Speenhamland and the New Poor Law of 1834, which seems to ignore Mark Blaug's work and much of the last decade's research on those topics.

JOHN BOHSTEDT
University of Tennessee

CHARLES KORR. *West Ham United: The Making of a Football Club*. (Sports and Society.) Urbana: University of Illinois Press. 1986. Pp. xiii, 257. \$24.95.

Charles Korr's study of West Ham United, a football club of East London, is an affectionate and well-executed piece of local history. The author had virtually unlimited access to documents and key participants, and the result is a convincing account of the internal operations of a rather anomalous type of commercial enterprise, including much about "what makes football different from any other traditional business" (p. 171).

Most of this book is devoted to a detailed examination of how a professional football club functions, develops, succeeds, and fails. The management must find and maintain both a physical plant and its principal asset, the players, while balancing the books. The management must deal with the fans, who expect a real investment in success, for example, expensive players, and the increased autonomy of players who, unlike their early predecessors, expect more than the honor of playing top-flight football for demanding employers. And the management must fend off the attentions of outside parties, including local politicians, who want to influence or benefit from the club's operations.

The author refers several times (pp. ix, 19, 27,

121, 143) to the relationship between the club and the community of which it is a part. "West Ham United," he says, "is an institution that gives people a sense of identity" (p. 41). But the book seldom brings this relationship into sharp focus. On one side are the club directors with whom the author identifies and to whom he ascribes a kind of seigneurial dignity. On the other side is a working-class neighborhood that remains nebulous. The real *dramatis personae* of this book are the men who run the club, and for this reason it is disingenuous of the author to call the club's thousands of local supporters its "real owners" (p. 27). On the contrary, as his accounts of the directors' firmness of character repeatedly demonstrate, the local supporters are nothing of the kind.

This study is local rather than social history, because Korr's forays into the larger historical and sociological issues dealt with by sports scholars are few and brief. Most satisfactory is the author's description of Arnold F. Hills, president of the Thames Ironworks, who founded the club in 1895 as an exercise in labor relations and benevolent social engineering. Elsewhere the author refers to, but does not explore, the familiar problem of "whether, and if so how, sports organizations reflect the society in which they exist" (p. 41), a topic to which local history would seem to be well suited. His point that the church and the football club are comparable institutions from a financial standpoint is interesting but accorded only a single paragraph (p. 52). There is a fleeting reference to football violence (p. 130), but the author does not mention that today West Ham supporters include some of the worst football hooligans in Europe, a point that could have served as an opening into the local culture of the West Ham community.

The author's brief chapter on "the employer-employee situation" is one of the book's more interesting sections. "The similarities between footballers and workers were obvious," he writes. "The sport developed in the context of an industrialized, urban society with players drawn mainly from working-class backgrounds, who mostly returned to labouring jobs after their time as footballers" (p. 171). Nevertheless, the working class remains an abstraction in this book, and it is significant that the author has left it to James Walvin to comment on "the squalid nature of football's industrial relations" (p. 177). On more than one occasion the author sounds like an alter ego of the club's management, and it is likely that this tone is related to his reticence about the uglier aspects of the class issue.

JOHN M. HOBERMAN
University of Texas

GERARD O'BRIEN. *Anglo-Irish Politics in the Age of Grattan and Pitt*. Dublin: Irish Academic Press. 1987. Pp. 231. \$55.00.

Gerard O'Brien has written a skillful and useful study of the activities of the late eighteenth-century Irish House of Commons based on a close reading of the available printed and manuscript sources and, especially, the debates in the *Parliamentary Register*. It is a worthy addition to the growing volume of work over the past quarter-century on Grattanian Ireland by such scholars as G. C. Bolton, Edith Johnston, Peter Jupp, A. P. W. Malcomson, Thomas Bartlett, and, above all, R. B. McDowell.

Early in the book, O'Brien makes clear that this is a study solely of Anglo-Irish parliamentarians, whether opposition Patriots, Dublin Castle administrators, or country gentlemen. It is not a study (as the title might imply) of the larger Anglo-Irish political community, much less of its more bizarre revolutionary offshoots. Neither the Church of Ireland nor Theobald Wolfe Tone is even mentioned in the text, and Lord Edward Fitzgerald and the United Irishmen rate only one cursory notice. As a good Peterhouse man, O'Brien is suitably contemptuous of any out-of-doors politics. Public opinion, he informs us, whether defined as pamphlet literature, parliamentary elections, or constituent pressure groups, played no important role in the period under discussion. "The sources indicate that government and opposition were influenced by circumstances and arrangements peculiar to the parliamentary arena" (p. 168). Only armed insurrection, or the threat of it, could effect this high politics equation.

The high point of O'Brien's analysis occurs in his revisionist account of the Irish Patriots of the 1780s and 1790s. He takes to task two centuries of historians who have extolled a united Patriot movement on the basis of high-sounding rhetoric in debate. On the contrary, O'Brien asserts, the party was as ill-defined and disorganized as the 1782 constitution it midwived. Riven with internal jealousies and strained personal relationships among the leadership, the party had few abiding principles to sustain it, O'Brien argues, and he successfully destroys the illusion of Patriot "comradeship and consistency" (p. 170). In fact, on the evidence presented, it is doubtful that O'Brien regards the Patriots as much preferable to the old Undertaker class of the pre-1770 period. Whenever they came close to real power—in 1788–89, during the Regency crisis, or in 1794–95, during the Fitzwilliam viceroyalty—the Patriots swiftly jettisoned any substantial reforming impulses such as the abolition of sinecures, the exclusion of placemen from the Commons, or parliamentary re-

form. Indeed, one might conclude that George III, Portland, and Pitt made a strategic error by overestimating the potentially revolutionary result of Fitzwilliam's Patriot alliance and his continuation in office.

O'Brien misunderstands Robin Reilly when he asserts, on Reilly's authority, that the cabinet ministers closest to Pitt destroyed the prime minister's letters. Also he continuously and irritatingly refers to that pompous and twice-appointed Irish lord Lieutenant George Grenville as the duke of Buckingham. The book is oddly proportioned for an ostensible study of the 1780–1801 period. By my count, only 16 percent of it deals with the 1790s. Finally, no concession is made to the uninitiated. Terms like "Renunciation Bill" and "Monks of the Screw" are thrown out without any explanation. Even specialists on eighteenth-century Europe or Britain will need their Lecky or McDowell handy before embarking on this text.

JAMES J. SACK
University of Illinois,
Chicago

FRANK NEAL. *Sectarian Violence: The Liverpool Experience, 1819–1914; An Aspect of Anglo-Irish History*. Manchester: Manchester University Press; distributed by St. Martin's, New York. 1988. Pp. xi, 272. \$55.00.

Most people in the world probably associate Liverpool with the Beatles, the former Merseyside musical group. In the British Isles, people think of Liverpool, the fourth largest city and former port of empire, as torn between urban decay and urban renewal. It also has a reputation for "Irishness" and turbulence. Events of the past decade—the summer riots of 1981, protests against the pope's visit in 1982, the violence of its soccer fans in Belgium in 1985—are but recent examples of Liverpool's history of civil unrest.

Frank Neal was born and reared in working-class Liverpool before the slums were cleared. His book traces the century-long rise in sectarianism from before the Irish Famine to the outbreak of the Great War. The work is based on a variety of primary sources, mostly city newspapers and Home Office papers, the Orange Order records being untraceable or nonexistent. Subjects covered are Liverpool's demographic development (the street maps are quite informative), in particular, the impact of the Famine Irish on the city; the rise of nativist "No Popery" working-class politics, including the growth of the English Orange Order, which was particularly popular in Liverpool and the surrounding county of Lancashire; and, from mid-century on, the endemic

communal strife within the city's working class, Protestant and Catholic.

Neal tells his seamy story with admirable balance and detachment. His writing is compelling and colorful. But he tends, especially in the last half of the book, to overwhelm his reader with details of the sectarian violence. One wishes that he had sacrificed some specifics for more analysis. In his "Postscript" (pp. 250–53), which substitutes for a conclusion, Neal remarks in passing that sectarianism developed because Liverpool lacked the paternalistic factory culture of Manchester; I would question the latter half of his assertion. And it will not do to refer to the "simple tribalism" (p. 252) of the two factions. To explain, one needs to investigate the activities of working people (Protestant and Catholic) when they were not head bashing and the actions of the employers, the tranquil middle and upper-middle classes who ruled the town. More analysis of the life of the town—its economy, local politics, union activities, crime, and police—would help explain the sectarianism in the streets. Neal's book must therefore be read in conjunction with Philip Waller's *Democracy and Sectarianism* (1981), a comprehensive political and social history of Liverpool, 1868–1939.

What Neal does demonstrate is the rise of militant Orangeism in nineteenth-century Liverpool. After 1850 the city was awash in provocative, and mostly Protestant, parades and processions; sectarian riots and rows at open-air meetings, church services, funerals, public houses, and schools; and domestic quarrels and miscellaneous other incidents between "papists" and Protestants. "Is Liverpool to become a second Belfast?" asked the *Liverpool Mail* in 1869 (p. 190). England's nearest city to Ireland, Liverpool after mid-century was about one-fourth Catholic Irish. Neal shows that the rise of a belligerent nativist working-class culture was a reaction to the Famine invasion of Liverpool and the implantation among "freeborn Englishmen" of Irish customs, Irish poverty, Irish crime, and Irish "disloyalty" in the form of Confederates, Fenians, and Home Rulers. With substantial help from Anglican leaders and Tory politicians, Liverpool's Protestant ships' carpenters, ropemakers, and warehouse workers developed a venomous anti-Catholicism that quickly came to define political legitimacy. The working-class Orange Order defended jobs and asserted English "rights and liberties" against the "evil" of popery's politics and religion.

A generation ago, two working-class youths, John Winston Lennon and James Paul McCartney, left Liverpool for a wider world. The leaders of the Beatles rock group left behind a culture no longer so overtly sectarian, but it was old and restrictive. Some said the Beatles were fashioning

a new, freer one based on "love." But, for still too many in Liverpool's working class, "a hard day's night" could mean some head bashing and cries for "Help!" Protestant and Catholic, they would not let it be.

STANLEY H. PALMER
University of Texas,
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PETER BROOKE. *Ulster Presbyterianism: The Historical Perspective, 1610–1970*. New York: St. Martin's or Gill and Macmillan, New York. 1987. Pp. xi, 259. \$35.00.

This tidy survey of Ulster Presbyterianism provides an excellent introduction to the subject and a useful reference work. Scholars working in Irish history, who need to look backward or forward from their own point of interest, will do well to keep this volume on their shelves.

Ulster Presbyterianism is a subdivision of Irish Protestantism. It was not endowed, like the Church of Ireland, but was linked to the government by a small subsidy—the regium donum. This subsidy was continued until the disestablishment of the Church of Ireland in 1868. This tenuous link with establishment gave Ulster Presbyterianism the character of a Scottish church in an Irish setting.

At the time of the plantation of Ulster, James I had imposed an episcopal establishment on Scotland so that the first Scots-Irish found themselves under the jurisdiction of the Church of Ireland. They remained Calvinist in spirit and were rescued from episcopal jurisdiction in 1643, the year that the English Parliament accepted the Scots Covenant in order to secure the support of the Presbyterian Scots against Charles I. Yet the Ulster Presbyterians, like their Scots counterparts, opposed the execution of the king in 1649. They were unhappy with the English Congregational form of Calvinism and never recognized the legality of the Cromwellian regime.

In spite of their royalism, the Ulster Presbyterians enjoyed a precarious existence during the Restoration, though they received the first installment of the regium donum in 1672. Only after the victory of William of Orange in 1690 did they mature as a church and organize the Synod of Ulster. This synod was separate from the Presbyterian organization in the south of Ireland, which followed an English rather than a Scottish pattern.

Brooke effectively analyzes in detail the differences between the various kinds of Calvinism in Britain, Continental Europe, and the United States. He does so to emphasize the differences between the Scots' form of Calvinism and its

manifestations elsewhere. Like the Scottish mother church, Ulster Presbyterianism was plagued by schism. The nonsubscribers, being the counterpart of the Scots seceders, remained outside the Synod of Ulster. They feared the influence of the state on the synod; nevertheless, they accepted the regium donum.

The author sees the chief difference between Scottish and Anglo-American Calvinists as the susceptibility of the Anglo-Americans to evangelical preaching, while the Scots and Ulster Presbyterians proved relatively immune to it. Brooke provides details of the various intrusions of the evangelical spirit into Ulster and its very limited influence. He suggests that this resistance resulted from the fact that the Scots and Ulster communities were self-sufficient. They felt no need to convert others.

Politically, the Ulster Presbyterians were inclined toward liberalism. They provided the vanguard of the Irish republican movement in the 1790s. Yet, being a minority, they took refuge in conservatism at the first sight of the Catholic majority in motion. The removal of political disabilities on the Presbyterian community in 1829 brought no rejoicing because the Catholics, too, achieved political equality.

When the threat of Home Rule emerged in 1885, most Presbyterians, like Church of Ireland Protestants, became permanent Unionists. Brooke argues that this development has distorted the shape of Ulster politics, that right and left divisions, characteristic of British politics, are obscured by Unionism.

This last section of the book is devoted to the rise of Ian Paisley. Brooke points out that Paisley's style of preaching, with its emphasis on personal conversion, is in the Anglo-American tradition of Calvinist preaching. Paisley's influence, he insists, is the result of the unwillingness of the mainstream clergy to take up the cause of militant Protestantism. The author concludes that "if Northern Ireland continues to be suspended in limbo between Catholic Ireland and secular Britain . . . 'bigotry' will have a very long future before it."

By any standard, Brooke has produced a sound, well-organized, and very readable book. There is certainly little written on Ulster Presbyterianism that is not found in his footnotes or listed in his bibliography.

HEREWARD SENIOR
McGill University

OLIVER MACDONAGH. *The Hereditary Bondsman: Daniel O'Connell, 1775–1829*. London: Weidenfeld and Nicolson. 1988. Pp. viii, 328. £ 16.95.

When I visited the grossly neglected ancestral home of Daniel O'Connell, Derrynane, in the early 1960s, I realized how little appreciated had been the life and work of the Liberator by succeeding generations of Irish republicans. In recent years, however, the house has been restored; scholars such as M. R. O'Connell have produced for us Daniel O'Connell's edited correspondence and other studies; and now we have this excellent biography of the great Irish statesman. The complex individual who wanted Irish freedom under the crown will probably never be appreciated by republicans, but they could all benefit from reading Oliver MacDonagh's sensitive interpretation of this passionate patriot.

MacDonagh is a bit more generous toward O'Connell than other writers have been. Sean O'Faolain, in his *King of the Beggars* (1938), had much to say about the "chameleon-like" quality of the man who was as trustworthy as a "Tammany lawyer," but MacDonagh, apart from an almost tedious recounting of O'Connell's financial affairs, glides over some of his other personal failings. He avoids discussion of O'Connell's legendary promiscuity. But he handles the matter of his dueling with aplomb, showing how no Kerry chieftain could accept the slur of poltroon for having avoided private mortal combat. MacDonagh knows well the era about which he writes and how every one of O'Connell's challenges was viewed as a contest between Orange and Green. When O'Connell dispatched the unfortunate Captain d'Esterre, Archbishop Murray was reported to have said, "Heaven be praised, Ireland is safe."

O'Connell had witnessed the French revolution at first hand, and he had no use for political violence such as Thomas Emmet's. He served during that tragic episode in a Lawyers' Artillery corps, joined the Kerry Yeomanry, and, during the famine year of 1822 when the Whiteboys reappeared in Iveragh, formed his own Yeomanry corps. Like his cousin-wife, Mary, he knew the peasantry well and feared their potential ferocity. He was never an "improving" landlord and did not hesitate to threaten those of his tenants in arrears with nonrenewal of their leases. Yet this biographer is wise to remind his readers that "we must be careful not to impose retrospective rigidities, the logic of a later century, upon him" (p. 55).

This most complex of men, who was convinced from an early age that one day he would "appear on the great stage of the world," never lost his "customary guile" (p. 35). He was arrogant enough to compare himself favorably with Henry Grattan, and MacDonagh believes that O'Connell's vision was "outward looking, or even imperialistic, at least in the spiritual sense" (p. 12). This

way of thinking can certainly be discerned in many of his statements from the time he allied himself with the priests, while Catholic Vetoists such as Richard Lalor Sheil, Thomas Wyse, and others accused him of building up a clerical caste with political ambitions. O'Connell himself protested that, although he was a sincere Catholic, he was not a papist, that he would as soon receive his politics from Constantinople as from Rome. Like Grattan, he hoped religious bigotry was a thing of the past. It was not only Sheil and Wyse who feared what O'Connell was creating, however, as his vision became more and more spiritually imperialistic (p. 12). Long before Thomas Davis, political liberals were uneasy about O'Connell, who proclaimed to Bishop Doyle his enthusiasm for the Catholic church: "I look solely to her advance" (p. 280).

This is an excellent study of O'Connell as an essentially eighteenth-century political animal, a "respectable, rationalistic, moral-force radical" (p. 273). As such he has left to our latter-day "patriots" a salutary reminder, as he did to the revolutionary forces of his own day: "the altar of liberty totters when it is cemented only with blood, when it is supported only with carcasses" (p. 53).

DESMOND BOWEN
Carleton University

JONATHAN DEWALD. *Pont-St-Pierre, 1398-1789: Lordship, Community, and Capitalism in Early Modern France*. (Studies on the History of Society and Culture.) Berkeley and Los Angeles: University of California Press. 1987. Pp. xx, 326. \$35.00.

Emmanuel Le Roy Ladurie concluded that the seigneurie was one of the essential frameworks of agrarian capitalism, and Albert Soboul asserted that the peasant plot, freed of the "feudal complex," was the locus of rural capitalism. Jonathan Dewald's case study of a Norman seigneurie over four centuries offers a third alternative. The bearers of agrarian capitalism were neither the barons of Pont-Saint-Pierre nor the peasants of the Andelle valley who had virtually disappeared by 1620 but rather the large tenant farmers and grain merchants of La Neuville closer to the main road from Rouen to Paris. Not that a Norman noble family such as the Roncherolles did not adapt their seigneurie to certain aspects of capitalism. Indeed, the conversion from fixed seigniorial dues to adjustable money rents on domain wood and grain land was completed by the late sixteenth century. Dewald concludes, however, that the seigneurs' "capacity to adapt was ultimately inadequate" (pp. 4-5). Like the *gros fermiers* of the Dijonnais, the market-oriented *la-*

boueurs and *marchands-fermiers* were the authentic capitalists in this part of rural Normandy.

This book is more than a case study about the relation of the seigneurie to rural capitalism. Dewald treats the larger issues of seigneurial power, local loyalties, and the responses of an old landed noble family to a whole series of challenges—political, economic, and cultural—from 1398 to 1789 and even beyond. In a splendid opening chapter, Dewald traces the gradual decline of the bourg of Pont-Saint-Pierre and the rise of its rival, La Neuville, on the other side of the seigneurial and royal forests of Longboel. By the late eighteenth century, Pont-Saint-Pierre had become a somnolent river town, inhabited by a cluster of rentiers, innkeepers, shopkeepers, notaries, and seigneurial agents handling a trickle of criminal cases in the seigneurial court. By contrast, La Neuville's tenant farmers were fully engaged in the sale of grain as well as investment in the expanding cotton and woolen industries of Elbeuf and Louviers on the other side of the Seine. The Roncherolles, however, partook of the tight and static world of Pont-Saint-Pierre.

Dewald identifies the elements of decline of seigneurial power and prestige, most of them apparent at least a century before 1789. The network of clientage and the dependence of lesser noble families on the Roncherolles atrophied after 1660. Although estate revenues rose after 1550, they did not rise as fast as those of the large tenant farmers until the last fifteen years of the Old Regime. The Roncherolles failed to reinvest in estate improvements or initiate new crop courses; the tenant farmers were the true agricultural entrepreneurs. The Roncherolles fought freedom of the grain trade by "policing" the local markets and limiting competition among grain dealers. They enforced their market tolls by blocking market forces. The cultural monopoly of the Roncherolles, as brokers to the wider world, was ended in the eighteenth century by increasing literacy among the population at large and by some pretensions to urbanity among the prosperous tenants and merchants of La Neuville. A "rural bourgeoisie" did indeed "rise" within the seigneurie of Pont-Saint-Pierre. Aside from ending seigneurial interference with the grain market, the revolution of 1789 did little more than confirm the steady erosion of noble influence in the countryside since the early seventeenth century.

Most impressive is Dewald's mobilization of serial data from fiscal and notarial documents to estate accounts and law cases. If the scarcity of personal correspondence prevents him from giving more relief to individuals, Dewald's sweep over four centuries more than compensates for this lacuna. It sets his book apart from family

studies such as my *The House of Saulx-Tavanes* (1971), which focus on the eighteenth century and place greater and perhaps undue weight on the "rupture" of 1789.

ROBERT FORSTER
Johns Hopkins University

JEAN-MARIE CONSTANT. *Les conjurateurs: Le premier libéralisme politique sous Richelieu*. Paris: Hachette. 1987. Pp. 286. 139 fr.

The early modern French nobility is currently among the most exciting historical topics under debate. In François Billacois's *Le duel dans la société française des XVI^e–XVII^e siècles: Essai de psychosociologie historique* and in a cluster of Anglo-American works in progress or in print by Jonathan Powis, Ellery Shalk, Kristen Neuschel, Donna Bohanan, Jonathan Dewald, Ruth Kleinman, and others, the nobility's once distrusted *mémoires* are yielding new (and often conflicting) secrets hidden from the memorialists themselves. We are introduced to nobles flaunting their individuality before Mars and Venus and other nobles undergoing the civilizing process. The old nobility of the sword is sometimes portrayed as distinctive in values and sometimes as linked professionally and socially with the newer nobility of the robe. In Queen Anne's household, the wives of aristocratic wielders of the marshal's baton held sway, but out in the provinces we are told that the brokers, wielders, and beneficiaries of power were a very mixed socioprofessional group. Given this state of scholarly uncertainty about sixteenth- and seventeenth-century French noble society, one might wonder what a study of the most conspicuous noble losers of the time could possibly offer in the way of enlightenment. Yet Jean-Marie Constant gives us much to ponder about the externally and internally changing destiny of the Chevreuses, Chalais, and Condés, male and female alike, who were born of the religious fervor and civil violence of the Wars of Religion into the statist world of Louis the Just.

Constant reads Retz, Tallemant, Campion, La Rochefoucauld, and other writers imaginatively to uncover a strain of Christian stoicism alongside the *dévo*t religiosity and noble pride of the Richelieu-haters who talked, debated, wrote, plotted, and yet failed to unseat or kill the cardinal-minister. The high principles of a Montmorency, and even of a Gaston of Orléans, are made to look more attractive than historians have made them out to be, and those among the second rank of noble players—Montrésor, Fontrailles, the three Campion brothers, and many others—are allowed to bare their souls right down to the hopeless

leftovers of the Cabale des Importants against Louis XIII's widow and Richelieu's successor. For those of us who know all of these grandees and gentry by name and reputation, and even for the more general reader who will occasionally be left dizzy by the dropping of names, there is the additional impressive feature of Constant drawing on his awesome genealogical knowledge to link the nobles' cloak-and-dagger moralizing with the early modern social trinity of family, friendship, and favor.

It would be a pity if potential readers of these narrative essays were put off by the author's reliance on *mémoires* for conspiratorial events, his strained linking of his "heroes" to the later "liberalism" of the book's subtitle, or his idealization of conspirators who were more often vengeful losers in the patron-client court game than genuine Brutuses or Hamlets. The important fact is that Constant gives us a novel look at complicated people within an increasingly disciplined culture, society, and state and provocative insights at every turn in his journey.

A. LLOYD MOOTE
University of Southern California

J. F. BOSHER. *The Canada Merchants, 1713–1763*. New York: Clarendon Press of Oxford University Press. 1987. Pp. viii, 234. \$48.00.

Although the title may suggest that this work is primarily a book about Canada, it is very much a work of French history, which sets the Canada trade in the context of the social and economic life of metropolitan France. J. F. Boshier argues that the structures of business and power and the personnel of the Canada trade have not been properly understood by many Canadian historians, who have often viewed the trade essentially from the colonial perspective. He objects to the provincialism of such analyses and especially to interpretations of the transatlantic trade and the commercial structure of New France couched in terms of class. He stresses, rather, the inseparability of religion, politics, trade, credit, and family as well as the sharp division of French commerce and finance along lines of function and religion.

Catholic officers, financiers, purveyors to the military, officials, clergy, and merchants dominated the government of New France and, until the 1740s, trade with the colony. The Catholic merchants were part of French society in a way that Huguenots never could be, and most had relatives in, and other connections with, the church and the complex official hierarchy of Bourbon France. The successful tended to buy offices and leave trade entirely. Huguenots, even

those who became leading bankers in Paris, were outsiders in this world of absolutism and Catholic orthodoxy. They had, however, extensive links to the powerful Protestant network of Atlantic trading society, whose centers were Amsterdam, Hamburg, and London. Some Huguenots, to live, marry, and do business in France at all, converted to Catholicism, but Boshier shows that many such conversions were nominal. Although he does not believe that it is possible entirely to ascertain the extent of Huguenot participation in French trade, he is able by extensive and imaginative research to reveal many Huguenot links among merchants who had outwardly conformed.

Delineating French trading and kinship networks, with particular reference to those engaged in trade with Canada, and adducing their significance are the book's main contributions. Here Boshier's findings generally confirm studies on the structure of the Atlantic economy by authors such as Jacob Price and analyses of public credit and finance by P. G. M. Dickson and Boshier himself. The book also offers further evidence of how the French monarchy's fiscal ineptitude and structural weaknesses forced it, notably after the early 1740s, to rely increasingly on Protestants and Jews to conduct its Atlantic trade and to finance the monarchy itself. Eventually, of course, the system collapsed, and France's North American empire fell, bankrupting many merchants, both Catholic and Protestant, and foreshadowing the ultimate collapse of the entire Bourbon system.

DOUGLAS MCCALLA
Trent University

ROGER METTAM. *Power and Faction in Louis XIV's France*. New York: Basil Blackwell. 1988. Pp. viii, 343. \$45.00.

The best thing about Roger Mettam's book is its title. Unfortunately, what follows is not of the same quality. Readers are told in the first paragraph of the introduction that the book is concerned with the mechanisms of power in seventeenth-century France, both the exercise of royal authority and the importance of social groups and institutions whose influence coexisted and conflicted with that of the crown. What follows is a survey of seventeenth-century French government at the national and provincial levels, although more emphasis is placed on the royal court and the government at Paris and rightly so. The book is organized into five chapters on the meaning of absolutism; the king and the nobility; social and political tensions, 1610–61; the personal rule of Louis XIV; and the domestic administration, 1661–1715. Major works published in these areas

during the past two decades are summarized, with original insights of the author interspersed. Unfortunately, much of the material is repetitive of the work of others with little that is new or original, and what is original has not been developed to any extent. In general a stronger evidential base is needed for the author's own insights. Perhaps the best chapter is on the personal rule of Louis XIV and his court, and I wish that Mettam would develop some of his observations in this chapter.

I think this book may be suffering from an identity crisis, and I was not sure exactly how to categorize it. Was it written for a general audience, for classroom use, for nonspecialists who are historians, or for scholars? The author may have confused his audience. The book was seemingly meant for general readers, or at least for an audience of historians who are not specialists in the field, because it thoroughly summarizes recent literature and covers all of the most important work. I was glad to see that Mettam makes a point of emphasizing the variety of studies available in English and does not concentrate solely on what the French have done, as is so often the case. He has used footnotes sparingly, however, not really enough to serve as an introduction to the field, and the book's lack of footnotes hampers its usefulness for undergraduates and nonspecialist historians. It is well written, and I enjoyed reading it. But its style and intellectual level are too sophisticated for most undergraduates and perhaps for most general readers as well. The book is written in an academic style, but it is not well developed enough to interest scholars in the field. It is a more scholarly version of the genre exemplified by *The Armada* of Garrett Mattingly, whose style appealed to a popular audience. Although the author has failed to write successfully in this genre, his attempt should be saluted: too few of us try to make specialized historical knowledge available to a wider audience.

SHARON KETTERING
Montgomery College

MARTINE SONNET. *L'éducation des filles au temps des Lumières*. Foreword by DANIEL ROCHE. (Cerf-Histoire.) Paris: Cerf. 1987. Pp. 354. 145 fr.

Although the eighteenth century is often called the Century of Enlightenment, little reflection from the *Lumières* penetrated into the schools where young Parisian girls were taught. If the education of girls was deficient, however, it was not because of a lack of facilities. By careful investigation Martine Sonnet uncovered over two hundred fifty girls' schools in Paris in the second

half of the eighteenth century. These schools, which provided enough places for around one out of three or four Parisian girls to get at least a few years of instruction, were diverse in organization, clientele, and cost. The range of institutions included the clandestine and unauthorized *écoles buissonnières* of the attics and apartments, schools that left traces in the historical record only when their mistresses' activities were challenged by the official schools run by the parishes and the cathedral, the congregational charity schools providing schooling for the poor, and the most prestigious of the *pensions* where the daughters of French and foreign aristocrats were sent for a few years of polishing.

All of these institutions tailored their curricula and conditions to meet the presumed needs of their diverse clienteles. But all offered what Sonnet argues to be a quite limited intellectual fare. Lessons emphasized preparation for one of three fates—the convent, marriage and motherhood, or apprenticeship into an “honest trade” for the small number who were expected to have to support themselves for at least some period of their lives. The reading of religious texts and recitation of prayers dominated instructional time in the whole range of institutions. Teachers strove to improve the skill of reading aloud with correct tone and pronunciation. More privatized or secular reading was either neglected or discouraged outright. The textbooks used reflected the particularly religious orientation of girls' education. For example, book inventories compiled during the investigations of clandestine schools (hence, not even officially religious in character) showed that nearly three-quarters of the volumes used to teach girls were religious or devotional, as opposed to just over half of the reading material seized from *buissonnières* that catered to boys. For girls, unlike for their brothers, the curriculum, length and age of school attendance, and progress in studies were all centered on the school's primary role of preparing young girls for their First Communion. Most pupils stayed less than three years, whether at a boarding or a charity school. Most entered and left by age ten or eleven. The range of pupils' ages was in any one institution quite broad, however, and efforts to discriminate among age groups or skill levels tended to be fairly crude, again pointing to the limited scope of the education offered. Even upper-class girls often stayed just long enough to prepare for communion, unlike their brothers, whose education in the *collèges* was more likely to continue into early adolescence, to last for five or more years, and to follow a structured classical curriculum.

What was offered beyond prayer and reading depended on the social origins and presumed

particular destiny of the pupils. The schools for the poor often devoted a great deal of time to the needle trades that most Parisian women relied on for income both before and after marriage. Particular skills—tailoring, lacemaking, and so forth—followed the vagaries of the market and the traditions of particular schools and teachers. Boarding schools offered a variety of enrichment courses, either as standard fare or “à la carte” (by private agreement between parents and teachers)—handwriting, arithmetic, grammar, music, art, dance.

The regulations that Sonnet has amassed and analyzed in order to discern the structure of daily life and the content of pedagogy in the girls' schools suggest austerity and discipline even in the most elite institutions. Life in the boarding schools was structured around prayer, regulated reading, orderly meals, and dormitory conditions designed to reduce intimate contacts among the girls and secure their chastity. Still, the rigidity that characterized the well-known Port Royal school appears in the light of Sonnet's evidence to be somewhat extreme. Most *pensions* were less rigid in their separation of older from younger pupils, less insistent on long periods of confinement within the institution, less controlling of their pupils' time. And the few existing memoirs that recount experiences in these schools further suggest that school life was less confining than the regulations would imply.

Sonnet concludes by noting that girls' education, shown to be rigidly status-oriented and extremely limited in terms of what it offered intellectually, was nevertheless praised by the investigators who visited these schools in the early years of the revolution. This observation suggests the irony of the title, for what satisfied the enlightened inspectors at the end of the Century of Light was, with respect to women's education at least, unmarked by the century's intellectual ferment.

Or was it? Sonnet would have done well to integrate her findings more fully into existing historical investigations of educational theory and institutions of eighteenth-century France and elsewhere in Western Europe. The existing feminist readings of Enlightenment thought regarding women and gender relations may also have provided an interpretive framework. Set in these contexts, Sonnet's findings could support the thesis that the girls' schools of Paris provided precisely what the Enlightenment prescribed and sought to turn out women according to a model that many of the *philosophes* would have found appropriate. But even without venturing outside the schools of eighteenth-century Paris, she offers us a privileged and richly documented entry into the sphere of girls and women in the late *ancien*

régime and some suggestions about how entering that sphere requires us to rethink our truisms about the past.

MARY J. MAYNES
University of Minnesota

NINA RATTNER GELBART. *Feminine and Opposition Journalism in Old Regime France: Le Journal des Dames*. Berkeley and Los Angeles: University of California Press. 1987. Pp. xviii, 354. \$38.00.

From its founding in 1759 until its suppression in 1778, the *Journal des Dames* passed through the hands of nine editors and assumed a succession of guises: initially a *rien délicieux* to entertain women, then an organ of boisterous feminism set on breaking conventions, followed by a more moderated social critique within the limits of the stylish, a celebration of Rousseauian intelligent motherhood, an across-the-board radical challenge to entrenched institutions, and, finally, again a bagatelle. The changeling journal's precarious life course, reconstructed in engaging narrative by Nina Rattner Gelbart, sheds new light on the Old Regime French press, the origins of modern feminism, and the prerevolutionary period.

Publications like the *Journal des Dames* that appeared with “tacit permission”—that is, screened by royal censors yet lacking the secure existence of an official “privilege to publish”—have been overlooked by recent historians of the press, who have assumed that politically significant content could appear only in wholly uncensored clandestine works. Yet Gelbart's first full-scale treatment of a tacitly permitted periodical reveals among these publications a tradition of “*frondeur* journalism,” which, like the Fronde, was both integrated with the established order and subversive, using legal channels to contest, agitate, and provoke change. Although hostage to a censorship system that shifted dramatically from permeability in periods of calm to repression in times of crisis, these publications nonetheless succeeded in articulating opposition to oppression of all forms, including misogyny, and pressing openly for the rule of law, religious toleration, freedom of the press, republican drama (against the corporate tyranny of the *Comédie française*), and Freemasonry.

The three editors of the *Journal des Dames* who were women pursued the interests of women as part of this more general fight against oppression. The first women to speak in a periodical on behalf of women and to women readers, they initiated a new phase of feminism. Whereas French feminism had earlier been theoretical and voiced either by men or, when voiced by women, to men, women were now “fighting for their own rights

through a periodical. . . . Theirs was more than a doctrinal feminism; it was a *féminisme d'action* of the *frondeuse* variety, with a direct impact on reality" (p. 93). These women's efforts and the obstacles they encountered tell much about "feminist and anti-feminist states of mind in France under the Old Regime" (p. 3). They also help explain why a feminine press did not develop smoothly in France in the eighteenth century as it did in both England and Germany.

Finally, Gelbart's work on the *Journal des Dames* offers information on the early careers of certain personages of the revolution whose works appeared in the *frondeur* press as well as insights into the long ripening of *frondeur* dissent into revolutionary mentality. Especially thought-provoking is her speculation that this embattled but effective feminine press may have laid the groundwork for women's unprecedented involvement in the revolution by stimulating women's participation in social issues and helping them find their own voices.

The book is brilliantly written. It is lively (even breezy) and dramatic (sometimes melodramatic, with clear villains and heroes). The narrative style, however, creates some problems. To make the story flow, Gelbart ascribes causes and imputes motives where sources appear not to provide them. She tells the story of the journal from the point of view of the journal, as the struggle of purposeful editors against a capricious establishment, rather than as a clash of two visions of the public good, each with its own rationale. Also because of its narrative form, the book does not pursue analytical approaches one has come to expect in histories of the press: content analysis, audience analysis, sociograms of the network of publishers whose overlapping involvements and joint strategies made up the *frondeur* press.

Still, this is an excellent work, based on extensive research in a rich array of archives. Gelbart strikes a good balance between biography, intellectual analysis, and institutional profile and successfully situates her single phenomenon within its multiple broader contexts. Like the journal that is its subject, this book is at once engaging, informative, and persuasive.

CAROLYN CHAPPELL LOUGEE
Stanford University

MONA OZOUF. *Festivals and the French Revolution*. Translated by ALAN SHERIDAN. Cambridge: Harvard University Press. 1988. Pp. xiii, 378. \$37.50.

Eighteenth-century encyclopedists and their establishment foes both attacked traditional festivals and called for new festivals. The French revolu-

tion carried through, purging the old festivals and prolifically creating new ones, a story that leading historians since Jules Michelet have made familiar. One of the many strengths of Mona Ozouf's excellent book is that it offers a critical interpretation of a great range of the festive, not just Paris events or famous pageants created by the likes of Jacques-Louis David. Her account of the Festival of the Federations, for example, includes the exciting journey that Bretons made to Paris—by foot—and their return home with gun salutes, oath takings, triumphal arches, fanfares, dinners, and balls in towns along the way.

The "Pantheonization" of Voltaire and funeral celebrations receive surprisingly little attention, but Ozouf's intent is not to survey or describe every festival through the decade of revolution. Always probing with a general issue in mind, she analyzes fetes in order to answer such questions as whether people celebrated primarily their own coming together in community (Emile Durkheim's emphasis) and who was excluded (often women, for example). She also asks whether excess and violence were essential elements, as Sigmund Freud and René Girard suggested. Thus, her study moves beyond categorizing the celebrations by their explicit themes, such as victories or the republic. As she shows in detail, one problem with using the official festival program is that the people sometimes improvised on their own and did not always perceive representations of, say, liberty and reason to be what the festival organizers intended. Ozouf also rejects the revolution's political divisions as her interpretative key. After assessing the rich political historiography (Alphonse Aulard, Albert Mathiez, Daniel Guérin, Albert Soboul), she goes beyond it to offer her own more anthropological understanding.

Where others have emphasized differences, Ozouf brings out consensus and continuity, a shared mentality. The revolutionary festival, whether created by one faction or another, was utopian, she stresses—an overly optimistic attempt to control, to unite and homogenize individuals. Festivals were efforts to reshape space and time, as she demonstrates in two highly innovative and suggestive chapters. Festivals were attempts to fulfill social and psychic needs for order, for community (she owes much to Durkheim), for the sacred.

Popular revelries not organized by Paris authorities are treated in a particularly important chapter that does much to explain the defensive and heavily controlled official fetes. Violence or simulations thereof, masquerades, and mockery characterized unofficial events, which were especially common during the fall and winter of 1793–94. Overall, in emphasizing the more common official festivals, the ones lacking parody and the car-

navalesque, Ozouf's account has the effect of slighting the socioeconomic and ideological reasons for the continuing subversion.

Her analysis succeeds brilliantly, however, in explaining the officials' taste for the Greco-Roman and their heavy-handed attempts to educate the people by such ceremonial. Refusing to present the revolution as historically developing and open to change, they preferred to fix it in tableaux of sacred beginnings. Fearful of overstimulated emotions and suspicious of the theatrical, the dominant revolutionaries resorted to austere allegory and verbose speeches. Their festivals were repeated attempts to close the revolution. And, as Ozouf shows in detail, they met with resistance from a large part of the population still attached to traditional feasts or not yet satisfied by revolutionary reforms.

This book originally appeared in 1976 in French and has been widely appreciated by specialists. What can an English translation do for additional readers now during the bicentennial of the 1789 revolution? The work assumes some background; it dates events by the revolutionary calendar and refers to such revolutionaries as Pierre-Joseph Duhem and Gilbert Romme without identifying them. Yet Ozouf's remarkable insights into the festivals and the revolution come through clearly to the benefit of any reader. Her new perspectives offer fresh ways of understanding the immense effort the revolutionaries made both to destroy the *ancien régime* and to perpetuate an emerging secular, liberal order.

CHARLES REARICK
University of Massachusetts

ROBERT B. CARLISLE. *The Proffered Crown: Saint-Simonianism and the Doctrine of Hope*. (Johns Hopkins University Studies in Historical and Political Science; 105th Series, number 3.) Baltimore: Johns Hopkins University Press. 1987. Pp. xii, 269. \$37.50.

In many ways Saint-Simonianism was bizarre, quixotic, and unique; in other ways it was deeply intertwined with the identity and destiny of modern France. Robert B. Carlisle successfully captures both of these qualities in his new study of the development of Saint-Simonian ideas and of the men and women who created a social movement based on Saint-Simonian "doctrine."

Carlisle's central purposes are three. First, he wishes to refute the received wisdom that there was a significant break between the ideas of Saint-Simon and the ideas of his young followers, many of them brilliant disgruntled outsiders in the society of Restoration France. Second, he disputes the

dismissive footnote that is often given to the movement, that is, that it had a few sensible ideas and a few important adherents who eventually came to their senses, in the process abandoning what had become a mad cult organized around the devious figure of Prosper Enfantin and capped by a fruitless quest in the East for the female Messiah. Finally, Carlisle attempts an apologia for Saint-Simonian ideology itself. He takes on its foremost critics and calls for a serious reexamination of Saint-Simonian efforts to "square the circle of individual and society" (p. 79).

Carlisle argues plausibly that the principal ideas of Saint-Simon's disciples were congruent with, if not entailed by, the theories of the master. Discussions of the role of the *industriels*, the social impact of technology, the movement of history, the importance of order and hierarchy, and the role of a new religion are all to be found in Saint-Simon's writings. But the real focus of Carlisle's study is the psychology and dynamics of the Saint-Simonian church. The story of the personal magnetism of Enfantin and of the schisms that afflicted the movement is here told in fascinating detail; parallels both to the later history of the psychoanalytic movement and to contemporary charismatic cults are striking. There are, however, some puzzling lapses in the narrative. For example, Carlisle supports his thesis that the successive breaks with Enfantin were not primarily defections of the practical wing of the movement (but rather complicated psychological separations) in part by citing the important role of the eminently practical Olinde Rodrigues in elaborating Saint-Simonian ideology. He refers only in passing, however, to Rodrigues's own separation from Enfantin and declaration of himself as the antipope.

Finally, the underlying theme of this book is that both Saint-Simon and his followers can offer us insight into the political problem of obligation in modern societies. But here the argument is much less convincing. One does not have to think of the Saint-Simonians as proto-totalitarians (a rather silly exercise) to have doubts about their moral and political sensibilities. They were not merely more naïve, more idealistic, and "bolder" in facing the consequences of the problem of modern politics than their contemporaries; one suspects that they never properly faced those problems at all. (Certainly Enfantin did not.) Similarly Saint-Simon himself fits rather oddly in the pantheon of those theorists who provide classic discussions of the balancing of freedom and authority. (Carlisle here mentions both Jean-Jacques Rousseau and John Stuart Mill.) Saint-Simon may have been a genius of sorts, but his talent was not for political philosophy.

Carlisle's first two aims, then, are ultimately

more successful than the third. Nevertheless, his book as a whole is an important contribution to the literature on Saint-Simonianism and on social reform in nineteenth-century France and can be recommended as well for the literacy of its prose and the liveliness of its style.

CHERYL B. WELCH
Harvard University

JAN GOLDSTEIN. *Console and Classify: The French Psychiatric Profession in the Nineteenth Century*. New York: Cambridge University Press. 1987. Pp. xiii, 414. \$49.50.

This book is a "metamorphosis" of a doctoral dissertation (1978) on the origins of French psychiatry in the nineteenth century. It resembles a dissertation only in its exhaustive documentation—there are well over a thousand footnotes—and in its tendency not to stray too far from the psychiatric cadres that constitutes its subject matter. In every other respect, this very useful book is a thorough and mature piece of scholarship and the best survey we are likely to have on the early psychiatric profession.

Jan Goldstein leaves no stone unturned in her effort to build a complete account of the institutional structures, professional strategies, and theoretical presumptions of this medical specialty. She has consulted numerous archives in Paris and the provinces with remarkable effect. She has discovered the limits of current nosologies in psychiatric practice, traced the lobbying efforts of the fledgling profession through the channels of bureaucratic and legislative action, uncovered the specialty's inner circles of power, and revealed the machinery of its system of patronage. She has also mastered the huge technical literature through which the profession established its claims for expertise and on which it based its therapies. Finally, she has provided convincing and learned explanations of the links between psychiatry and the state, intellectual life and popular culture, and the Catholic church, its most powerful rival in the business of curing "souls."

There are some findings here that are entirely original and many that deepen our previous knowledge. Goldstein shows how the "moral therapy" developed by Philippe Pinel and his followers meets many of Thomas Kuhn's criteria for a paradigm "shift" in scientific practice, particularly in the way it spread throughout the profession, building on a body of knowledge in general medicine but decisively replacing older theories and therapeutic modes. Throughout, she emphasizes the essential eclecticism of the movement. Thus, moral therapy and the "psychological" assump-

tions on which it was built did not rule out a "physiological" medicine of nervous lesions and internal secretions. Nor did the professionalization process cut practitioners' ties to the healing "arts" of medical charlatans or cause them to repudiate the spiritual consolations traditionally provided by religious healers, although, inevitably, as Goldstein reveals in her concluding chapter, the latent conflict between clerical and professional treatment of the mentally ill broke into a full-scale crusade by anticlerical psychiatrists and their political allies in the positivist environment of the early Third Republic. Finally, in the most imaginative parts of the book, Goldstein explains how the "monomania" and "hysteria" diagnoses may be usefully regarded as social constructions of psychiatric knowledge that served certain organizational, political, and cultural needs of the profession.

Although this book will be read with profit by generalists and French historians, the unswerving attention to detail will make it chiefly interesting to specialists in the history of medicine and the professions for whom it also provides an example of interpretive tolerance and common sense. If anything, this doctrinal latitudinarianism is taken too far, for it conceals the extent to which this flourishing field of study—part history of culture, science, and institutions—is permeated by epistemological and historiographical debate. Goldstein cites what is immediately useful or uncontroversial and multiplies primary sources in preference to engaging in methodological argument with historians or thinkers holding different points of view. The reader is thus presented an apparently seamless product, which loses in liveliness what it gains in its quality of synthesis.

ROBERT A. NYE
University of Oklahoma

PATRICK H. HUTTON, editor. *Historical Dictionary of the Third French Republic, 1870–1940*. In two volumes. New York: Greenwood. 1986. Pp. xvi, 585; 588–1206. \$125.00 the set.

From Action française to Jean Zyromski, Patrick H. Hutton's compilation marches through seventy of the richest years of French history and some seven hundred fifty major political figures, novelists, feminists, labor leaders, artists, intellectuals, institutions, and cities. It contains contributions by over one hundred fifty scholars, among them some of the best historians of modern France. As Hutton himself notes in his preface, this is the only comprehensive reference work of this kind to date, and it is an admirable and masterful effort to

combine between two volumes an enormous amount of information.

There are some very good essays indeed, such as David Troyansky's entry on aging, which touches on regional variations, demographic trends, and the social and legal implications of the fact that France was the first nation to experience a significant aging of its population by the beginning of the twentieth century. Kathryn Amdur provides as succinct a summary of anarchism and its influence on the French labor movement as one could have hoped for. Gary Cross offers an excellent entry on immigration, and Michael Marrus adds one on France's Jews. Patrick Bidelman sums up the major trends of French feminism in four dense but highly readable pages, and Karen Offen provides solid surveys of the women's movements for political and civil rights, attitudes toward women, and women in the labor force. In addition, there are twenty-four long interpretive essays treating the economy, society and social stratification, literature, the labor movement, architecture and urban planning, and music, to name a few.

As might be expected in any work of this breadth, the essays are uneven in quality. R. L. Williams's portrait of Gustave Courbet is disappointing in its assertion that Courbet's ideas about art and politics merely reflect an "extended childhood," especially for anyone familiar with T. J. Clark's *Image of the People: Gustave Courbet and the Second French Republic, 1848-1851* (1973). The main points of the Charte d'Amiens should have been spelled out in the entry on that subject (or in the entry on revolutionary syndicalism). And there is the occasional error. Ernest Ferroul did not attempt to unionize winegrowers; his run-in with Clemenceau occurred because of his leadership of the winegrowers' revolt of 1907, which just might have been the largest peasant rebellion of the Third Republic, if not in French history.

Although it would be unfair to condemn omissions in a work of this already enormous scope, one is nonetheless struck by the presence of an entry on industry but not on agriculture, one on Madeleine Pelletier but not on Louise Saumoneau (who is nonetheless mentioned in Biddleman's entry on feminism). Whereas women in the labor force and the attitudes of French socialists toward women are noted by Offen, there is no entry for women in the labor movement, nor do the entries on those subjects mention women at all, despite the fact that there is now a burgeoning literature on both. In spite of this, Hutton's historical dictionary is arguably an invaluable reference work that will be useful not only to French historians who have forgotten what the Rif rebellion was, or who

Lafargue, but also to French scholars in language, art, and literature and, of course, to students.

LAURA LEVINE FRADER
Northeastern University

HERMAN LEBOVICS. *The Alliance of Iron and Wheat in the Third French Republic, 1860-1914: Origins of the New Conservatism*. Baton Rouge: Louisiana State University Press. 1988. Pp. 219. \$25.00.

In this book, Herman Lebovics explores what many historians now view as the key event in French politics in the late nineteenth century: the creation of a stable, conservative "Republican consensus" that blocked working-class revolution and preserved the social and economic supremacy of the upper classes. Drawing on his expertise in German history, Lebovics takes pains to show that what happened in the early Third Republic was similar to what happened in Otto von Bismarck's Second Reich in that the "great depression" of 1873-96 generated a resurgence in protectionist sentiment that brought republican industrialists and monarchist landed aristocrats together in an "alliance of iron and wheat" comparable to the "iron and rye" alliance in Germany. In seeking higher import duties, both parties to the alliance, Lebovics argues, were motivated as much by social goals as by economic hardship. The aristocratic landlords were seeking to head off radicalization of the peasants; the industrialists were seeking to defuse the growing militancy of their workers. Both, Lebovics believes, got what they wanted—and more—in the Méline tariff of 1892. The republican-monarchist rapprochement forged in the battle for tariff protection became a permanent ruling coalition whose parliamentary power was subsequently reinforced by the Catholic church's *ralliement* to the republic, while the policies of Jules Méline's government of 1896-98, which Lebovics dubs the "Ministry of Social Pacification," solidified the coalition's hold over the rural and urban masses.

Sound in its overall contours, Lebovics's argument is open to question in some of its parts, for example, his use of the "social protectionism" concept, so prominent in the current historiography of the German empire, to explain why French industrialists favored a return to high protection. By stressing the industrialists' fear of rising labor militancy in the early 1880s, Lebovics ignores the fact that certain manufacturers had always been protectionists and had been seeking to end the trade treaties and to restore a high legislated tariff throughout the 1860s and 1870s. Moreover, while the French industrialists doubtless wanted to curb labor unrest and the threat of socialism from the

early eighties on, it is also true that higher import duties offered at best a circuitous route to that end. Only if the industrialists had in some way passed their gains from higher duties on to their workers, perhaps in the form of higher wages, would tariff protection have served the purposes of social pacification. To be sure, Méline and his friends talked of this linkage in parliamentary speeches, but this was just talk, designed to win support for a policy that was going to benefit a few manufacturers at the expense of many consumers (including the workers). To really make his case, Lebovics needs to draw on the company files and prefectorial reports in the Archives Nationales to show that increased protection in 1892 did in fact allow specific firms to give their workers higher wages and other benefits, thereby turning them away from socialist politics. Without such evidence, his assertion that French industrialists sought higher import duties mainly as a solution to labor troubles remains largely unsubstantiated.

Despite shortcomings in his handling of the tariff question, Lebovics presents a forceful argument on the origins of the "new conservatism" that unquestionably enhances our understanding of the convoluted political history of the Third Republic. For that, the book deserves the attention of scholars.

MICHAEL S. SMITH
University of South Carolina

DAVID JAMES FISHER. *Romain Rolland and the Politics of Intellectual Engagement*. Berkeley and Los Angeles: University of California Press. 1988. Pp. xiii, 378. \$32.95.

In the first work on Romain Rolland to be published in English since 1971, David James Fisher reintroduces an almost forgotten author. Fisher has a complete command of the vast corpus of Rolland's writings and analyzes them judiciously. His biographical treatment is admiring yet nuanced, a balanced and scholarly study by a trained historian, not hagiography in the manner of Stefan Zweig's 1920 portrait.

For a half-century Rolland was intensely involved with social and political actuality. His commitment went far beyond the single act for which he is usually remembered, the publication of "Above the Battle," his impassioned appeal calling for an end to the slaughter of the Great War, penned from self-imposed exile in Switzerland in 1914. Rolland knew personally and frequently debated publicly with practically every major intellectual of his day. He had private contacts with an extraordinary range of political figures, from Gandhi to Stalin.

Rolland's energy was prodigious and his imagination fertile. His fascinating proposal for an "Intellectual's International" and his "Declaration of Independence of the Mind," both drafted in 1919, are carefully discussed by Fisher. In some areas Rolland set precedents for future intellectual engagement, as in his refusal of the Goethe Prize in April 1933, probably the first case of a writer renouncing a literary award as an act of protest against the regime or organization offering it.

What is most original in this patiently researched and clearly written intellectual biography is the way Fisher employs the case of Rolland to provide new understandings of the much-debated question of the political engagement of intellectuals, which has been especially controversial in France. Fisher never neglects the rapidly shifting historical contexts that surrounded and to some degree impelled Rolland's activity. He argues convincingly that the trajectory of Rolland's intellectual engagement involves five discernible "languages" or positions, from the "oceanic sensibility" that inspired his involvement in the People's Theatre movement of the 1890s to his uncritical fellow-traveling of the late 1930s. At this point in his career Rolland was a strong supporter of the French and Spanish Popular Fronts. Unlike André Gide, he never publicly articulated his private doubts about developments in the Soviet Union, including the Moscow purge trials. Rolland did intercede discreetly on behalf of individuals and was able to obtain the release of Victor Serge from a Soviet prison camp.

Although Fisher does not take his argument quite this far, the evidence that he assembles, much of it gleaned from Rolland's voluminous correspondence, suggests that Rolland was perhaps the quintessential engaged intellectual. He experimented with every imaginable variety of liberal and leftist activity except for actual membership in a Communist party (which some would construe as no longer engagement but rather *embrigadement*).

This work of impeccable scholarship contains an openly admitted polemical thrust. Fisher believes that there is still a place in our world for the politically committed writer, and he uses the case of Rolland to demonstrate his conviction. In an age of *dégagement*, such a position is refreshing.

DAVID L. SCHALL
Vassar College

LUC BOLTANSKI. *The Making of a Class: Cadres in French Society*. Translated by ARTHUR GOLDHAMMER. New York: Cambridge University Press and

Editions de la Maison des Sciences de l'Homme, Paris. 1987. Pp. xviii, 397. \$49.50.

In the popular press, French industrial organization is usually described in terms of two stereotypes. The first is a firm operated by a beret-capped *patron*, the owner of a venerable family enterprise, who works closely with his employees, attends their baptisms, weddings, and funerals, and enjoys their trust and respect. The second stereotype is that of the highly organized industrial proletariat engaged in class warfare with the bourgeoisie. Neither of these images accurately describes contemporary French industrial organization, Luc Boltanski argues, because of the rise of a new social class, the *cadres*.

The concept of a *cadre* as a group of individuals responsible for directing the operation of an organization and the indoctrination of new members is not new, of course, but before the 1930s the term was mainly applied to career military personnel. Events of the late 1930s and 1940s, however, created circumstances that permitted the *cadres* to emerge as a dominant force in French industrial life. For the most part, the *cadres* can trace their origins to the strikes of 1936 and the elevated status that the Matignon Accords of the same year gave to the Confédération Générale du Travail (CGT). Middle-class salaried groups, led by engineers, organized themselves to combat what they viewed as the "dictatorship of the CGT" (p. 45), its influence over the Popular Front, and the disruption of production caused by conflicts between employers and workers. The Vichy government's Charte du travail officially recognized the *cadres* as a civilian institutional force, and after the liberation in 1944 managerial employees led by engineers formed the Confédération Générale des Cadres. Lower-echelon *cadres* such as sales people became "shock troops in the service of the engineers" (p. 83).

The key to the emergence of the modern *cadre*, "the versatile new manager" (p. 103), was the Marshall Plan. Although the Marshall Plan is generally viewed in terms of transfers of capital equipment from the United States to Europe, the transfer of values and attitudes toward work and production was an essential component of the plan. Along with the machines went American industrial psychology and scientific management techniques, all part of a political plan designed to defuse dangers from both the working-class supporters of communism and the bourgeois supporters of fascism. Leaders such as Jean Monnet, Pierre Mendes-France, and Jean-Jacques Servan-Schreiber, the founder of *L'Express*, "the magazine for *cadres*" (p. 113), devoted themselves to implementing the principles of American scientific

management in France. Success came rapidly, and by the 1960s *cadres* had become a distinct, legally recognized social group with its own values, mores, and traditions that occupied a central place between the working class and the bourgeoisie.

Boltanski's book is one that can be read on two levels. First, it contains much of use to the traditional historian of contemporary France. Its sections on the history of management, the ideological components of the Marshall Plan, and the role of higher education in forming an administrative elite are excellent. Second, for those interested in the application of social theory to historical research, Boltanski provides a wealth of ideas that will stimulate investigation. His arguments that class formation is subjective rather than objective, that a social class can form around an idea of itself as defined by its members' educational credentials and professional tasks, and that an administrative elite can make both workers and owners of capital secondary players in the postindustrial world are stimulating and susceptible to testing in a variety of historical settings. It is essential to remember, however, that the orientation of the book is not social history but social theory. Readers with a firm grounding in the vocabulary and methods of the social sciences may approach Boltanski's book with confidence, although they will find it challenging. Those without such training may find it inaccessible.

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ANTHONY CLAYTON. *France, Soldiers, and Africa*. Elmsford, N.Y.: Pergamon. 1988. Pp. xxv, 444.

In *France, Soldiers, and Africa*, an essay in "imperial history," Anthony Clayton surveys France's military activity in Africa from the naval landings in Algiers in 1830 to the airborne support missions to former French colonies in the 1980s. Clayton first describes the nature of the French military system in Africa and its structural connections with the national army and French society. He then examines the system "at work" by following the course of empire, defining and evaluating the European and native forces engaged in Africa as well as in French military operations in Europe and other parts of the world.

Like Caesar's Gaul, the French army was divided into three parts—the metropolitan army, the Armée d'Afrique, and the Troupes coloniales (La coloniale). The two overseas forces had clearly defined turfs: the Armée d'Afrique raised European and native troops in North Africa, and the Troupes coloniales (also known as the Troupes de marine) consisted of similar forces procured ev-

everywhere else in the empire, including Africa south of the Sahara. Regardless of their home base, however, the empire armies served wherever they were needed. Among the European forces, the Zouaves and the Foreign Legion are the best known and most celebrated; among native troops the North African and Senegalese *tirailleurs* (sharpshooters) and the Moroccan *goums* (irregular forces) won recognition on battlefields across the globe and fought with particular distinction in Europe during the two world wars.

Nearly half of the present-day states of Africa were defined by French military power. Clayton acknowledges the primacy of brute force in the acquisition and expansion of French Africa and the discrepancy between the ideal and the reality in the empire system. Marshal Hubert Lyautey, the greatest of France's African proconsuls, who professed respect and concern for the native peoples of North Africa, routinely employed military terror and economic warfare against any and all who opposed him. He was convinced that the ends justified the means. At times the ends were impressive. With pacification came road and railroad construction, irrigation canals, and dramatic progress in health and education. Clayton recognizes these as positive military contributions to African social, economic, and political development.

Well into this century fighting under the tricolor conferred personal dignity and worth on native African soldiers and made real an intercultural comradeship of the battlefield. Loyalty to France was in large part loyalty to the institution of the French army itself—its pride, professionalism, paternalism, and record of combat victories. Clayton's excellent and well-written book explores all those ties that bound. And he leaves no doubt why they died.

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ANGEL ALCALA, editor. *The Spanish Inquisition and the Inquisitorial Mind*. (Atlantic Studies on Society in Change, number 49.) Boulder, Colo.: Social Science Monographs or Atlantic Research and Publications, Highland Lakes, N.J. 1987. Pp. x, 680. \$65.00.

As stated in the introduction to this volume, two principal factors appear to account for the fairly recent surge of research on the Inquisition: the reappearance in the twentieth century of repressive forms infinitely more cruel and sophisticated than those of the original Inquisition and the substitution for the Franco regime in Spain of a society with stability but expanding horizons. An

even more persuasive stimulus might be that almost everything remains to be done to remove the Inquisition from the realm of myth, whether pro or con, and horror story in which it has remained so long.

Thirty essays are included in this collection, all assembled (with some violence to content) around the cause, structure, and operation of the Inquisition and Spanish culture, spirituality, and politics. A final section called "Inquisitorial Mind as a Historical Constant" concludes the volume. This last section contains some odd bedfellows: Martin Luther, John Calvin, Sebastian Castellio, Mary Tudor striving for uniformity, Enlightenment views of the Inquisition and the persistence of the inquisitorial mentality, and a final essay by B. Escandell that quite justly reminds us that we have witnessed in such charming phenomena as the KGB and the Gestapo things far more revolting than any prototypical inquisitor could possibly have imagined.

Almost every issue in Spanish history for the past four centuries appears to be subject to challenge, probably because the scientific use of documents as well as their inaccessibility still remain barriers. B. Netanyahu challenges the familiar thesis that the Inquisition was a response to Judaizing tendencies, but he uses terminology in such a way as to cast doubt on his findings. He refers to sixteenth-century Spain as a totalitarian state but subsequently admits that such a phenomenon was impossible anywhere before the twentieth century. His argument is that Judaism was in retreat on all fronts, the Jews having for the most part become sincere Christians, so that the establishment of the Inquisition could not have been a reaction to any danger from crypto-Jews. In virtually all of the contributions (T. F. Ruiz and B. Benassar, for instance), we are confronted with not an apology for the Inquisition but an assertion of its relatively benign character, in contrast to contemporary civil procedure and the fact that the functioning of the Inquisition was characterized by scrupulous regard for legal niceties.

There is a noteworthy paper by G. Henningsen in which we are given the useful information that the Inquisition archives are now in Madrid, not Simancas. Using a quantitative approach he has discovered that the Inquisition was more active against Old Christians than either Jews or Moriscos and that the pursuit of witches was as enthusiastic in Spain as it was in other European countries. He suggests a fertile area of research in the Portuguese Inquisition where the difficulties of research are partially solved because all of the relevant documents are preserved in the Torre do Tombo.

A paper from the editor of the book, A. Alcalá,

dives into the question of precisely how inhibitory the Inquisition was to intellectual development in the Siglo de Oro. He concludes that, although a great many foreign books were prohibited and a secret framework of allusions had to be adopted by authors, there was no real suppression of literary talent so long as writers observed the conventional standards established by the inquisitors. J. Pérez Villaneuva has an essay rich in insight into the character of Philip IV, and, something almost inevitable, E. Peters has investigated the background of C. H. Lea's research and publications on the Inquisition as well as his inquiry into the celibacy issue.

Most of the salient topics of Spanish historiography related to the Inquisition receive passing attention: humanism and its stunted growth (V. Pinto and Alcalá), the spirituality of the Alumbados, curiously nonmystical in character despite appearances (J. C. Nieto and M. Andrés), and the theory of the Two Spains, the last topic with a more or less pervasive presence in over half of the papers. But it is surprising to find almost no trace of Ferdinand Braudel's argument that one of the reasons that Spain turned in upon itself was a genuine threat from Islamic imperialism in the Mediterranean.

One person did most of the translations, which read smoothly. On page 315 we find Menseguy for Mésenguy. Galicia sometimes appears as Galacia, and conjunctions and prepositions frequently have letters transposed. In short the proofreading leaves something to be desired. There is no purpose in including illustrations so miserably reproduced that they appear as splotches of black and white. There is a list of contributors given but no index.

SAMUEL J. MILLER
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RICHARD POPKIN. *Isaac La Peyrère (1596–1676): His Life, Work, and Influence*. (Brill's Studies in Intellectual History, number 1.) New York: E. J. Brill. 1987. Pp. x, 241. \$56.25.

Richard Popkin presents Issac La Peyrère as an idiosyncratic, original, and highly influential thinker. La Peyrère was born a Calvinist in Bordeaux just before 1600, but his family name, milieu, and ideas suggest that he may have been the kind of Christian whom contemporary Spaniards called *converso*, when they were being minimally correct, and *marrano*, or pig, when they were being ugly. Many families with Jewish ancestors had left the Iberian peninsula for the north, congregating in the string of ports that began with Bordeaux.

Converso was an antiassimilationist label intended to mark lineages, to the nth generation, as suspect in faith and inferior in blood, La Peyrère, in Popkin's view, was probably one of the writers who accepted this imputed status and transformed it into a virtue. He awarded pride of place in the end of days that he saw coming to Jewish Christians, syncretists *avant la lettre*. As soon as Christianity shed its anti-Jewish side, the remaining Jews could convert. They would assemble in France, regrouping for Jerusalem, while the rest of the world would in its turn become Christian.

Because La Peyrère could intimate but hardly reveal Jewish origins and proclivities, Popkin's interpretation is necessarily speculative, though quite persuasive. Not that other *marranos* gave evidence of endorsing La Peyrère's ideas. The descendants of converts, as Popkin notes, must have had a range of attitudes toward their predicament. Given the passing of time, and the world around them, most would surely have preferred full recognition as the Christians they now were. It may be helpful to reserve the term *Marrano*, in the upper case, to those persons who wanted to remember a Jewish past and who tried to see their present suffering as purposeful.

La Peyrère's foreshortening of the future led him to stretch the past back to eternity. The original, lawless humanity had existed in a wealth of nations long before God began again, with Adam, and created the Jews. The Amerindians proved to La Peyrère that Genesis was only a local beginning. He took advantage of a trip to Scandinavia to become an expert on the Eskimos. He also argued that Moses could not have written the Bible's first five books.

La Peyrère constricted the scope and divinity of the Bible in order to compress its force by demonstrating that the life of the Jews, and the Christian sequel, composed the yeast that would soon give rise to an immensely variegated world. His ideas, taken together, sounded like a phantasmagoria, though they delivered shrewd blows to beliefs that were increasingly hard to rationalize. And his circle of acquaintances included writers whom we take seriously. Richard Simon, the great biblical scholar, was a friend and critic. The reviewer, index in hand, who tries to pin down the specific influence of La Peyrère on others has a bit of a hard time. Popkin is making connections rather than showing direct and commanding influences. He does convincingly re-create an intellectual environment that bred radical views and even more radical refutations.

One later writer who clearly did use La Peyrère was the Abbé Gregoire, champion of Jewish emancipation in 1789 and herald of the millennium. Other intellectual heirs were less estimable. Vol-

taire turned the argument of man before Adam into a diatribe against the Jews. Racists claimed that blacks had been separately and unequally endowed. Popkin astringently traces the noxious sequels to the thoughts with which La Peyrère had innocently and optimistically played. But surely the author of a "Marrano theology" already knew how well ideas could serve as prisons.

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University of Pittsburgh

CHRISTOPHER WHITE. *Peter Paul Rubens: Man and Artist*. New Haven: Yale University Press. 1987. Pp. 310.

Yale University Press has produced a book that is a model of its kind, that is, the coffee-table book. The lavishness of the book's layout, with more than three hundred illustrations in color and black and white, is most impressive. Christopher White's text is large-minded and presented with minimal apparatus—the notes refer mostly to primary sources, which in Rubens's case have been well published. The author is adept at high popularization, which the English have always done better than anyone else. His prose is fluent and intelligent, written by an educated man addressing an educated and intelligent audience. He is master of his subject, and few will come away from his account of Rubens's life and career uninstructed or unpleased.

The emphasis of White's text tends to fall more upon Rubens the man than the artist, but this does not mean that his art is given short shrift. White's approach is doubtless in response to the growth of interest in the social position of artists, and it is easily adaptable to the familiar form of historical biography. As a result we have a more rounded view of Rubens than used to be the fashion. Given his status as one of the leading citizens of Antwerp, an artist and diplomat who received knighthoods from England and Spain, and a student and collector of Roman artifacts whose opinions were respected by the best scholars in Europe, the view of Rubens in White's narrative is an especially interesting one. The author is clearly drawn to these subjects, and the accounts he gives of Rubens's diplomatic activities in the period of the Thirty Years' War, as well as of his antiquarian studies, are especially welcome in the context of a general biography.

Only occasionally do White's own prejudices penetrate the smooth surface of his text, most noticeably when questions of allegorical expression and meaning arise. Thus, he writes that it would be a mistake to expect a tightly defined theme in the famous series of paintings on the life

of Marie de Médicis, given the changes introduced as the series progressed. Yet Rubens was thoroughly conversant with the themes and techniques of ancient encomiums, and the allegorical inventiveness for which he was celebrated was fortified by a real expertise in the political symbolism of Roman coins and gems. Particular alterations that he made affect the cycle's meaning (which is remarkably unified and very Latin) no more than the revision of a verse affects the meaning of a panegyric poem or play intended to set the career of an individual prince in the context of a universal moral and political discourse. To take another example, White finds the oil sketch for the equestrian portrait of the duke of Buckingham preferable to the final painting, not because the brushwork is more direct but because "the finished version was encumbered, possibly at the duke's request, with more allegorical figures in flight around the duke, diminishing the vitality and spontaneity of the original image" (p. 190). But, if we have now learned to value the exuberance and sensuality of baroque art as something spontaneous and vital, surely we should not shrink from its characteristic high-flown forms of symbolic expression, which in fact contribute much to the magnificence of effect that the author acknowledges in the Medici cycle.

These are minor quibbles. White is an excellent cicerone for anyone interested in becoming acquainted with Rubens either as a man whose life and character were extraordinary by any measure or as the greatest of artists in a century famous for its virtuosos.

CHARLES DEMPSEY
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JANET L. POLASKY. *Revolution in Brussels, 1787–1793*. (Mémoires de la Classe des Lettres, Collection in-8°, second series, number 66.) Hanover, N.H.: University Press of New England, for the University of New Hampshire or Académie Royale de Belgique, Brussels. 1987. Pp. 315. \$25.00.

This book is interesting both as a substantial contribution to the history of Belgium, where it has already won recognition, and as a stimulus to studies of eighteenth-century revolutionary movements in general. Janet L. Polasky has attempted the difficult task of combining historical narrative with sociopolitical analysis. Her work is thus a reinvestigation of the resistance of Brussels, the central city of Brabant, to the innovations of Emperor Joseph II and of its participation in the winning of Belgian independence from Austria in the two revolts of 1789 and 1792. The third phase of the conflict, the repudiation of the French in

1793, is similarly but less exhaustively considered. Polasky's familiarity with recent studies in economic history enables her to show that the area was more industrially advanced than has been supposed, a fact that influenced the composition and character of the two parties principally involved, the "traditionalists" and the "democrats." Correlation between the ideologies of these parties and the social sources of their strength is apparently constant; only the Jacobins of 1792-93 emerge as newcomers and exponents of an essentially alien gospel. Polasky, therefore, contends that the battles of the two principal parties, whether against Austria or France or each other, are neither a pale reflection of the French revolution nor manifestations of bucolic backwardness. On the contrary, she argues, the party conflicts were distinctive and progressive.

Although the author's sympathy for the democrats is evident, her work is as well balanced as it is clearly expressed. The reiteration of contemporary arguments may be criticized, particularly as extensive paraphrasing sometimes causes confusion between the opinion cited and the interpretation given to it, but here our universities, which seldom distinguish between a thesis and a book, may be more at fault. Certainly this study is firmly founded on extensive archival research and meticulous annotation, and, if adjacent areas, particularly the United Provinces and Liège, seem less important in this study than they were, that perspective may be accepted as the consequence of concentration on a region and a theme.

More generally, this book affords interesting sidelights on French foreign policy, although it is strange to see the decree of *fraternité et secours* presented as a warning that French support might be withdrawn. Polasky's interest lies rather in the question of the relationship between the French and Belgian revolutions. She argues convincingly that the Belgian revolution was indigenous in the area she examines, yet she also suggests that, because social conflict is apparent in both countries, the "orthodox" (that is, socialist) interpretation of the French revolution may be reconcilable with the conception of a widespread democratic upheaval. In fact, some of the features she notes as peculiar to Brabant have also appeared in recent (and "unorthodox") studies of French society. Yet Polasky's traditionalists and democrats seem to have more in common with each other than with revolutionary France. They apparently desired national independence and an appropriate constitutional regime above all else, and even their frequent references to the sovereignty of the people do not seem to have the really revolutionary connotation of absolute democratic right. That is not to say that social antagonisms are unimpor-

tant, that different revolutions may not have some causes in common, or that the people of Brussels and Brabant were really counterrevolutionary. The suggestion is simply that social analyses are unlikely to transcend ingrained differences in national attitudes.

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LUC COURTOIS. *L'Introduction des étudiantes à l'Université de Louvain: Les tractations préliminaires (1890-1920); Etude statistique (1920-1940)*. (Travaux de la Faculté de philosophie et lettres de l'Université Catholique de Louvain, series 33, section d'histoire, number 7.) Louvain-la-Neuve: The University. 1987. Pp. xxx, 193. 760 F.

This book includes two studies: the debates from 1890 to 1920 concerning the admission of women to the Catholic University of Louvain and the patterns of female attendance at the university from 1920 to 1940. Luc Courtois used mainly the archives of the University of Louvain, especially the papers of Mgr. Ladeuze, rector of the university from 1909 to 1940, and enrollment statistics.

Belgian women were first admitted to higher education at the University of Brussels in 1880, followed soon thereafter by Liège and Gand. Catholics, led by Ladeuze, began to discuss admitting women to Louvain only during the years immediately preceding the First World War. Although Catholic educators proved willing, the bishops were not persuaded until 1920, forty years after women had been admitted to Brussels. Belgian Catholics were reluctant to abandon the traditional view of women as wives and mothers, and they feared mixing the sexes at any educational level.

Once admitted in 1920, the number of women students at Louvain grew at a rate averaging 0.5 percent a year to 160 students or almost 10 percent of the student body by 1940. At first a fairly high percentage of the female students were older than average and independent of their families, including a number of nuns and foreign students, but as time went on the female students became typical in age and middle-class background of female university students everywhere in Belgium. Although over 60 percent of the students were Flemish in origin (30 percent Walloon and 10 percent from Brussels), most (five-sixths) at first chose instruction in French. By 1940, however, this was changing, and over half of them were being instructed in Flemish. It is interesting in this regard to remember that Louvain was the first Belgian university to introduce courses in Flemish.

The second half of the book involves a very detailed analysis (thirty-eight tables) of the female student body from 1920 to 1940, including rates of growth, age and origin of the students, subjects studied, and comparison with male students and with women at the universities of Brussels, Liège, and Gand. Unfortunately, Courtois was unable to find sufficient data on the social origins and career choices of students. His studies do tell us that enrollments of women students grew faster than those of men and were less susceptible to economic downturns. Young women received better grades and were more likely to obtain a diploma than were male students, all of which suggests that the women came from more elevated social origins. The rate of growth in female enrollments at Louvain was slower than at Brussels and Gand (7 to 9 percent). By 1940, less than 10 percent of the students at Louvain, 14 percent at Gand, 18.5 percent at Liège, and 25.7 percent at Brussels were women. This suggests that Catholic families continued to be reluctant to send their daughters to the university. Other than that, the young women behaved like middle-class women in any university at the time; they favored liberal arts and pure sciences and avoided such fields as applied science and engineering, law, medicine, and theology, doubtless because all of these professions restricted female access to some degree or another.

Although the study is carefully researched and the many tables are expertly assembled and clearly presented, the book fails to provide much information that is new. The debate concerning the belated admission of women to Louvain is of some interest, and the statistics are useful to specialists on the subject of women and higher education. Generally, however, the book lacks focus. The author fails to discuss the role of women in student life and their struggle for acceptance in a conservative Catholic university. Courtois admits this weakness and projects a future book on the subject. But, by rushing into print with the present study, he presents a curiously disjointed work; the first half, on the general issue of the admission of women to Catholic higher education, has only a loose connection with the statistical account in the second half. The projected but uncompleted third part would have tied together the first two sections and given the book a unity and interest that it now lacks.

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A. HEERDING. *The History of N. V. Philips' Gloeilampenfabrieken*. Volume I, *The Origin of the Dutch Incandescent Lamp Industry*. Translated by DEREK S.

JORDAN. New York: Cambridge University Press. 1986. Pp. xiii, 343. \$47.50.

Several first-rate books on the invention of electric lighting systems (a critical component of which was the high-resistance incandescent light bulb) and the industry created in the wake of this technological achievement have been published recently. The best of these is Thomas P. Hughes's *Networks of Power: Electrification in Western Society, 1880-1930* (1983), but there is also Robert Friedel and Paul Israel's *Edison's Electric Light: Biography of an Invention* (1986). The first volume of A. Heerding's projected three-volume history of the Philips company, originally published in Dutch in 1980, is a worthy addition to this literature. It is an especially appropriate (though methodologically less ambitious) complement to Hughes's comparative work, which focuses on the United States, England, and Germany but which contains no mention of the Netherlands.

Philips today is a major multinational firm producing a variety of electrical and electronic products. This first volume in the series treats the late nineteenth-century origins of the company, which began as a father and son partnership producing high-quality incandescent bulbs in a single factory. But this book is much more than a company history. Since the volume actually concludes shortly after the establishment of the Philips lamp works at Eindhoven in 1891, its focus is more on the comprehensive development of the lighting industry in the Netherlands and is thus even broader in scope than the subtitle indicates.

The book contains six well-crafted chapters, each of which is capable of standing as an independent essay. The first is a judicious discussion of the invention of a commercially viable incandescent light (in which the Dutch played no role) and the companies formed to exploit the invention. Although Edison receives due credit, the important accomplishments of Joseph Swan, St George Lane-Fox, and others are not neglected. In the second chapter the author takes up the history of the Philips family, which had been engaged in the tobacco trade since the early eighteenth century. Gerard Philips, son of the merchant, cigar manufacturer, and banker Frederik Philips, was the key figure in the establishment of the lamp works. Although trained as a mechanical engineer and subsequently employed by a shipbuilding firm in Scotland, he had become fascinated with electricity by the mid-1880s. He pursued this interest by enrolling in courses at Glasgow University. He then worked in a series of jobs (with the Anglo-American Brush Electric Light Corporation in London, with Seel and Company in Berlin, and as the Amsterdam representative of Allgemeine

Elektrizitäts Gesellschaft) that ultimately prepared him for entering the lamp-making business in partnership with his father. The third and fourth chapters deal with the development of the lighting industry in the Netherlands and the craft-based lamp producers who preceded Philips. The fifth chapter is a fascinating account of the battle for the municipal lighting franchise in Amsterdam. The book concludes with a chapter on the conception and birth of the Eindhoven lamp works, which was an immediate success.

This is strict narrative history. To the extent that there is a theoretical structure, it is of the "I opted to explain the birth of the enterprise in terms of economic, technical and social factors" (p. xi) variety. The story certainly is well told, detailed but never dull (at least to one with some interest in the industry), but the absence of a theoretical structure (even one as loosely defined as in Hughes's work) does present some problems when it comes to evaluation rather than description. It is never made clear, for example, why Philips was so much more successful than its Dutch predecessors and why it soon rivaled its foreign competitors. Perhaps this will be addressed in volume 2.

WILLIAM J. HAUSMAN
College of William and Mary

CHRISTINA CARLSSON. *Kvinnosyn och kvinnopolitik: En studie av svensk socialdemokrati 1880–1910* [Perceptions of Women and Women's Politics: A Study of Swedish Social Democracy, 1880–1910]. (Arkiv avhandlingsserie, number 25.) Lund: Lund University. 1986. Pp. 323. 130 KR.

Christina Carlsson has written a very interesting book about the issue of women and their political and social needs in Sweden during the period 1880–1910, specifically as these needs were perceived in the labor movement in Sweden during these years. The book thus deals with most of the burning questions facing all labor organizations in Europe at this early stage of the movement: for example, is there a separate women's issue in the labor movement, or can history and politics only be understood in the context of class politics? Can women be emancipated as a gender, or can this emancipation only take place inside the broader enhancement of the working class as such? What is the position of women as mothers and workers, spouses as well as colleagues of male workers? Can these two distinct roles be combined for women? And what is the main role of women—that of class-based producers or that of reproducers and chief protectors of the new generation?

The debate over these basic issues took place in the context of the specific socioeconomic develop-

ment underway in Sweden during the last two decades of the nineteenth century. The process of industrialization brought more and more women into the urban proletariat, while the old practice of piecework at home continued on an expanded scale. The question of the role of women in society and in the labor movement therefore assumed concrete forms: Should women be remunerated at the same rate as men? How could one prevent a process of pauperization, whereby women and children undersold men in the economic marketplace by accepting lower wages? How would the expanded use of female labor affect home life, the position of the children, and the private relationships between men and women? Were there certain occupations or work hours that should be removed from female activity?

The budding working-class movement in Sweden was confronted with these philosophical and practical questions, and they turned out to have a divisive impact on both the Social Democratic party and the trade unions. Some of the most radical women in the party and the unions considered the need for complete equality indispensable, and they severely castigated those of their male counterparts who refused to accept this view. Many males, in turn, accused these females of setting class considerations aside in favor of the more parochial issue of women's rights. Indeed, these male voices intimated that the women of the Left accepted the arguments of women from different socioeconomic classes more readily than the views of their own class comrades of the opposite gender. This kind of argument fed back into the women's organizations and split them. Thus, the Swedish labor movement remained severely divided on this issue throughout the thirty-year period under examination in the book.

Three major political issues emerged as the chief battleground in this continuous debate, namely, the issue of the right to work for women, the question of equal pay for equal work, and the main tasks of women in society. On the question of suffrage, the dominant forces in the Social Democratic party and the trade unions (predominantly men) argued that insistence on suffrage for all, regardless of gender, would delay the possibilities for early male suffrage. This became the official position of both the party and the unions. Female suffrage did not come about until after the end of World War I.

The question of equal pay for equal work also raised a number of controversies. Important elements of the political leadership on the Left argued that such a principle would lead to a general depression of wages, while others felt that cheap female labor would represent a threat to all work-

ers. As we know, equal pay is still not instituted in Sweden or anywhere else.

Finally, the question of the real role of women in society produced much controversy in the Swedish Left during the period under examination. The most radical elements demanded that marriage must be discarded and that children should be raised by public institutions, such as kindergartens. Relationships should be based on free love, not personal or economic subjugation of women to men. The mainstream of the labor movement rejected this position and declared its devotion to the family and motherhood. Official social democratic policy thus was devoted to the institution of policies that could strengthen the proletarian family. To further this goal, the mainstream leadership sponsored laws to remove women from night work and certain hazardous occupations.

The left wing, especially many of the women's organizations, rejected this position and demanded absolute equality in the marketplace. Again the moderate mainstream prevailed, proving the essentially reformist character of the Swedish labor movement. In this respect, the Social Democratic party and the trade unions remained in the mainstream of the international working-class movement in this period, a point discussed in considerable detail by the author.

Carlsson has written a good book. The body of the material is presented within the parameters of an analytical framework that furthers systematic examination. A great deal of work has been done in original sources, thus establishing the empirical value of the book. The personal biases of the author may at times emerge (on the side of feminism), but this is probably inevitable in any study that deals with controversial subjects. Besides, it should be understood that the particular line of thought toward which the author leans has considerable merit, at least as one analytical way of examining the questions at hand.

The book has an analytical index and thorough footnoting. I would recommend it to all those who can read Swedish (there is also a good summary in English).

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JOHANNES ERICHSEN and MICHAEL HENKER, editors. *"Vorwärts, vorwärts sollst du schauen . . .": Geschichte, Politik und Kunst unter Ludwig I.* Volume I, *Katalog zur Ausstellung*. (Veröffentlichungen zur Bayerischen Geschichte und Kultur, number 8/86.) Munich: Bayerische Staatskanzlei. 1986. Pp. 348.

JOHANNES ERICHSEN and UWE PUSCHNER, editors. *"Vorwärts, vorwärts sollst du schauen . . .": Geschichte,*

Politik und Kunst unter Ludwig I. Volume II, *Aufsätze*. (Veröffentlichungen zur Bayerischen Geschichte und Kultur, number 9/86.) Munich: Bayerische Staatskanzlei. 1986. Pp. 551.

JOHANNES ERICHSEN and URSULA HUBER, editors. *"Vorwärts, vorwärts sollst du schauen . . .": Geschichte, Politik und Kunst unter Ludwig I.* Volume III, *Schauspiele von König Ludwig I.* (Veröffentlichungen zur Bayerischen Geschichte und Kultur, number 10/86.) Munich: Bayerische Staatskanzlei. 1986. Pp. 446.

In 1986 Bavaria celebrated two significant anniversaries: the centennial of King Ludwig II's death and the bicentennial of King Ludwig I's birth. The centennial anniversary generated great popular enthusiasm, for Ludwig II, that "fairy tale king" who built flamboyant castles, subsidized Richard Wagner, and died early in mysterious circumstances, remains a source of endless fascination (not to mention tourist industry revenues). The bicentennial for Ludwig I (Ludwig II's grandfather), on the other hand, was a more sober affair, inspiring few commemorative beer steins and music boxes but considerable scholarly interest. It was also the occasion for an ambitious exhibition on Ludwig's reign titled *"Vorwärts, vorwärts sollst du schauen . . .": Geschichte, Politik und Kunst unter Ludwig I.* In connection with this event, the Haus der Bayerischen Geschichte published a handsome catalogue of the exhibition, a collection of Ludwig's plays, and a volume of scholarly essays devoted to Ludwig's place in Bavarian political and cultural history. The following review is concerned only with the last of these productions.

Franz Josef Strauss recently hailed Ludwig I as "Bavaria's greatest king" (no doubt he meant crowned king). This was not saying a great deal, for Bavaria's kings—the first installed by Napoleon and the last ushered out by revolution in 1918—were not a terribly impressive lot. Nevertheless, Ludwig I was the greatest royal builder and art patron among the German princes of the nineteenth century, and he did much to make Munich the most handsome of the larger German cities. His significance as royal statesman is less firmly established, though in this regard, too, he left a lasting imprint on the Bavarian scene.

The essay collection under review here admirably attempts to embrace the entirety of Ludwig's contribution to Bavarian history. Its first three sections focus primarily on Ludwig's political legacy, the role of his advisers, and his relationship to the German national unity movement. The concluding two sections are devoted first to Ludwig's sense of history and the practice of historical scholarship during his reign and then to his role as royal builder and art patron.

In a competent opening essay on Ludwig's political and historical ideals when he was still crown prince, Eberhard Weis emphasizes the negative impact that the French revolution and Napoleonic expansionism had on the young man's perspective. Ludwig more or less consistently opposed the alliance that Count Montgelas and Ludwig's father, Max I, made with Napoleon; Ludwig's fervent desire was to join other Central European states in a "war of liberation" against the French "satan." So deep was his hatred of imperial France that he even pledged to drink no more wine while Bavaria remained allied with that nation. Eventually, of course, Ludwig got his wish, and Bavaria turned against its erstwhile ally just before the Battle of Nations in 1813. In the interim Ludwig had begun to combine his Francophobia with a hatred for the Enlightenment and a cultivation of things medieval, which formed the basis for his lifelong Romanticism.

Part and parcel of Ludwig's Romanticism was his passion for the study of history, which he believed could be the intellectual and spiritual source of Germany's cultural and political regeneration. He got this idea partly from the historian Johannes von Müller, whose crucial influence on the young prince is the subject of a fine essay by Ralph Marks. As monarch, Ludwig brought the Austrian historian Joseph von Hormayr to the University of Munich, where he postulated (with Ludwig's blessing) the ennobling unity of "patriotic history," national literature, and art. Hormayr's chief ally in this endeavor was another royal adviser, the poet Eduard von Schenk. The influence of both of these men is skillfully examined in two essays by Sylvia Krauss.

Regarding Ludwig's relationship to the German national unity movement, the next group of essays elucidates the central paradox of this monarch's reign: although he liked to think of himself as the "most German of Germans" and talked incessantly of "Teutschland," Ludwig did little to foster German unity. As Walter Schmitz explains, it turned out that his patriotism was essentially nostalgic and cultural, and he was unprepared to sacrifice Bavarian sovereign rights on the altar of unification. Moreover, Ludwig distrusted nationalistic sentiments that were not orchestrated from above. Thus, he would have no more truck with the Frankfurt Parliament of 1848 than would his Prussian counterpart Friedrich William IV, who was also unprepared to accept a crown "from the gutter."

Insightful as this volume's discussion of Ludwig's political career is, it neglects one of the more important (and certainly more colorful) chapters in his reign, namely, his brief and explosive liaison with Lola Montez. As the king's "best friend" and

informal adviser, this self-proclaimed Spanish dancer created such a scandal in Ludwig's court that the king was almost forced from the throne by popular protest. Lola was to Ludwig I what Wagner was to Ludwig II. Surely she deserves more than casual mention in a comprehensive treatment of his reign.

Ludwig's role as builder and art patron is subjected to exhaustive scrutiny in this volume, which is only fitting given the importance Ludwig himself accorded to this aspect of his rule. To get a sense of Ludwig's accomplishment in this area, we need to recall that his building program embraced, *inter alia*, the Danube-Main Canal, several of Munich's most notable museums and churches, the Ludwigstrasse-Leopoldstrasse complex, the restoration of the Bamberg Dom, the Kursaal in Kissingen, and the Walhalla Monument near Regensburg. Few would dispute the artistic and technical success of most of these endeavors. But the Walhalla Monument—perhaps the most bombastic example of patriotic *kitsch* south of Emperor William II's *Siegesallee*—showed (like Ludwig's plays) the limits of this monarch's artistic imagination.

The late Max Spindler notes in his contribution to this volume that there is as yet no satisfactory biography of Ludwig I. This being the case, the present collection of essays is especially welcome.

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HARTWIG BRANDT. *Parlamentarismus in Württemberg 1819–1870: Anatomie eines deutschen Landtags*. (Handbuch der Geschichte des Deutschen Parlamentarismus.) Düsseldorf: Droste. 1987. Pp. 898. DM 198.

Hartwig Brandt brings another notable addition to the distinguished series of handbooks on German parliamentary history edited by Gerhard A. Ritter. As one of the nineteenth century's outstanding representative bodies, Württemberg's diet warrants this extended study, which details its development as a link between changing governmental institutions and evolving social and political structures. Although important scholarship on pre-1870 Württemberg exists, we have lacked anything remotely satisfactory for understanding the complex of issues treated here.

Although of venerable ancestry, the post-1815 constitutional parliamentary system was no restoration of the old regime but the beginning of a new politics, one whose transforming rhythms from 1819 to 1870 resonate with themes familiar to other German states. Thanks to Brandt we can now examine this process close up, giving life and

substance to these often-neglected decades. His frequent comparisons illuminate more than just Württemberg's history.

Brandt divides the volume into five parts: elections, the organization and procedures of the diet at work, the diet as legislature, state finances, and the development of political parties in relation to royal government. The last topic, building on the first four, takes up one-half of the book. Each subject is handled with a richness and a vitality that carry the reader through a vast, but never dry, narrative and is combined with analyses of institutions, political practices, ideas, laws, and much more. We also find periodic deft portraits of individuals and a consideration of the social status of participants in politics.

Brandt's sustained and encyclopedic examination of all aspects of parliamentary life for the entire half-century builds an effective frame for comprehending the dynamics of the period. Although the complexity of the texture belies dramatic turning points, the years 1831, 1848, and 1859 indicate rough benchmarks. During the elections of 1831, political associations sprang up throughout the state, the standard ban on political associations notwithstanding. After that candidates were more apt to be chosen for their political views than for their social standing; local officials' control over the process was eroding. In consequence, the government became a "party"; it always won but had to fight the opposition publicly and persuade it in the diet. Brandt deals with 1848–49 less as a revolutionary episode that failed than as a movement with political results, especially the split between liberals and democrats. *Altständische Liberalismus* was gone, and the diet had to confront new issues. Its monopoly over political discussion was challenged by the press, and political Catholicism emerged, to name a couple of these issues, while the government had honed its instruments of domination.

The national movement after 1859 largely developed outside of the diet (constitutionally, the discussion of foreign affairs was a royal prerogative), although it soon engulfed all other issues. King Karl's accession to the throne in 1864 further accelerated change when he reduced censorship, recognized the right of free association, and authorized other reforms. Brandt concludes that Württemberg could have developed a parliamentary government in time, although it should be stressed that his is a history of what happened rather than what did not. Nor does he write with an eye on the events of 1866 and 1870, though, of course, these enter the story at the end.

Brandt's eight hundred pages may not get the readership they deserve, but they will be indispensable. Nearly three thousand footnotes to the

literature, to printed and unprinted sources, provide anyone working in German history between the first and second empires with a mine of information. The consummate fusion of the conventionally distinct genres of narrative, analysis, and reference is not what one normally expects in a handbook, but the thorough indexes make it possible to use it that way, too.

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RICHARD J. EVANS. *Death in Hamburg: Society and Politics in the Cholera Years, 1830–1910*. New York: Clarendon Press of Oxford University Press. 1987. Pp. xxii, 676. \$98.00.

In the early morning hours of August 15, 1892, a construction worker from the Hamburg suburb of Altona became violently ill on his way home. The attending physician, noting the patient's spasms of vomiting and diarrhea, suspected that he suffered from Asiatic cholera, but this diagnosis was not accepted by senior medical officials. For several days, even as similar cases were reported in and around the city, the authorities resisted sounding the alarm. By the time the dreaded public announcement was made, a full-scale epidemic was under way. Between August 15 and November 12, 16,956 inhabitants of Hamburg contracted cholera; 8,605 died.

These catastrophic events stand at the center of *Death in Hamburg*. The book's first three chapters trace the emergence of the social, political, and ideological environment in the city during the late summer and autumn of 1892. The fourth chapter provides a chilling account of the epidemic itself; the fifth measures its uneven impact on the city's population; and the sixth examines its long-run significance for Hamburg's politics and society. Richard J. Evans tells us a great deal that is of interest about the etiology and course of cholera, as well as about public health and biological research, medical organization, and sanitary reforms. But his real concern is with the social forces that were shaped and revealed by the character of the disease's victims and of those who fought it. In his words, "Sanitary reform, like other aspects of urban reform, stood at the centre of a complex interplay of economic, political, and ideological forces. It is the exploration of these forces, and their relationship with one another, that forms the principal justification for its study" (p. 110).

Nineteenth-century Hamburg was dominated by a commercial elite that defined its interests according to what Evans calls "the commonsense dogma of the primacy of trade" (p. 39). Often

innovative and energetic in its economic activities, this elite ran urban affairs through a set of old-fashioned institutions in which traditional prestige and amateur enthusiasm counted for more than professional expertise and administrative experience. As a result, "the structures of politics and habits of mind that dominated the administration of Hamburg in the second half of the nineteenth century were ill-suited to the circumstances of a major European city" (p. 107). At the same time, Hamburg's dominant classes came to live in a world socially, culturally, and spatially separate from that of the proletariat, which had to bear the burden of the city's inept government, polluted atmosphere, and unhealthy water.

The epidemic of 1892 showed with painful clarity the often-fatal consequences of institutional weakness and social inequality. Local authorities were not only slow to respond to the epidemic's outbreak but also divided and uncertain about what course to take once action had become unavoidable. "Our system," declared the president of the Property Owners' Association in October, "is hopeless. This fact is worse than the epidemic itself" (p. 384). And nowhere was the system's failure more apparent than among the poor, who made up a disproportionately large number of the cholera's victims. Evans shows in compelling detail that, although a complex set of variables determined one's chances of contracting the disease, behind this complexity was the fact of social inequality, "the structurally determined poverty which was such a striking feature of Hamburg society in the late nineteenth century" (p. 466). Cholera struck hardest and lasted longest among the city's least fortunate—harbor workers, domestic servants, and the denizens of rundown slums. When finally confronted by the consequences of their failure, Hamburg's leaders recognized the need for reform: the water supply was purified; public health improved; and some administrative changes were made. More basic alterations, however, in Hamburg's politics and society came—if at all—as the result of powerful popular protests such as the great strikes of 1896–97. The cholera shook but did not shatter the authority of the city's social, economic, and political establishment.

Evans titled the concluding section of his final chapter "Hamburg, Germany, and the World," thereby evoking Percy Ernst Schramm's classic *Hamburg, Deutschland und die Welt: Leistung und Grenzen hanseatischen Bürgertums in der Zeit zwischen Napoleon I. und Bismarck* (1943). The differences between Schramm's and Evans's books mark off the distance between two different eras in the historiography of German urban life. Whereas Schramm wrote as an insider who had to distance himself from his subject with an affectionate irony

reminiscent of Thomas Mann's *Buddenbrooks*, Evans views the city through the eyes of a sympathetic but skeptical outsider. Schramm is concerned with the urban elite's political ideas, cultural values, and national sympathies; Evans wants to explore the whole social structure, with particular emphasis on the sufferings and privations of ordinary men and women.

Evans's volume is an important work that provides us with a broadly conceived and richly detailed account of a nineteenth-century city's inner life.

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CATHERINE M. PRELINGER. *Charity, Challenge, and Change: Religious Dimensions of the Mid-Nineteenth-Century Women's Movement in Germany*. (Contributions in Women's Studies, number 75.) Westport, Conn.: Greenwood. 1987. Pp. xx, 205. \$29.95.

This is a very important book because it focuses on the mid-century period and is therefore a major contribution to what has been an area of marked neglect in the history of the German women's movement. Only a modicum of studies has been published in German on this period, and there are few works in English. Catherine M. Prelinger's study is based on extensive research in East and West German archives, and future researchers should be able to take advantage of her finds. And she was able to use a broad array of obscure published works. Prelinger proves that the process of the reclamation of sources for women's history is still under way, and, although it is a challenge to uncover those rare documents, she demonstrates yet again that, with extensive original research, pioneering discoveries can be made. The sources do exist. Moreover, with her balance between archival research and her use of published sources, Prelinger has come upon many hitherto unknown women who did not publish, and, thus, little is known about them. At the same time the study offers new insights into such well-known figures as Amalie Sieveking, Louise Otto, and others, who published extensively. This study also adds weight to the argument that it is necessary to push back the date of the emergence of an organized feminist movement in Germany. Although the book does center on women's groups in Hamburg, Prelinger makes a convincing case that a number of these organizations had branches in many cities and that there was widespread support from individuals all across Germany.

The scope of Prelinger's work is accurately described in the title of her monograph; she has studied the significant interconnections between

women's charitable work, women's education, religious values and motivations (especially those of the religious radicals), and the development and evolution of the women's movement. Therefore, this work is also an important introduction to women's participation in religion, a subject largely ignored by contemporary German feminists. Prelinger seeks to understand the impact of those religious values on women's rationale for participation in public life. She argues that "the feminist agenda of the religious radicals was very specific, at least as it was voiced by its most prominent spokesperson: it summoned women to enter the field of charity and to transform it into one of social reform, to feminize and deconfessionalize early childhood education, and to secularize and expand women's higher education so that women could accomplish these goals" (p. ix).

Despite the number of women who became involved, and the organizations that were created, the movement fell victim to the reaction following the failure of the revolution of 1848. In fact, Prelinger offers a whole new dimension to our understanding of the early 1850s, as she makes a strong case that the reaction had a gender component to it. Thus, although a significant women's movement developed early on in Germany, it ultimately suffered from the victory of conservative, if not reactionary, forces. Once again it is clear that research into women's experiences has important ramifications for history as a whole. As a final note, I would like to add that Prelinger provides a valuable bibliography and a useful index.

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JACK R. DUKES and JOACHIM REMAK, editors. *Another Germany: A Reconsideration of the Imperial Idea*. Boulder, Colo.: Westview. 1988. Pp. xi, 250. \$32.50.

This is the proverbial curate's egg of a book. It contains ten essays on aspects of imperial Germany, together with a "Summing-Up" by Joachim Remak and a substantial annotated bibliography. Although fairly randomly selected, the topics do address some major issues of German history in this period and fall under several headings: army, state, and society (Dennis Showalter on the army as a national institution and Jack R. Dukes on the Army Law of 1913); the idea of the city and municipal administration (Andrew Lees on middle-class civic pride and Peter Merkl on the "urban challenge"); social aspects of religion (Ronald Ross on Catholic backwardness and Michael Phayer on the organizations of Protestant and Catholic wom-

en); and the universities (Alan Beyerchen, "On the Stimulation of Excellence in Wilhelminian Science," Charles McClelland on the general character of the university system, and Konrad Jarausch on American reactions to the same). Eleanor Turk contributes a single essay on press reactions to the Labor Regulation Bill of 1899. Some of the essays are very useful. Beyerchen's discussion of the distinctive institutional nexus of innovation linking industry, government, scientific entrepreneurs, and individual academics, which allowed German science to overcome the inertia of the universities and both service the economy and achieve high standards of research, is easily the most original and substantial. Turk adds another valuable empirical piece to her earlier essays on related subjects. Ross argues convincingly that Catholic disadvantages resulted more from the economic backwardness of Catholic regions than from active policies of discrimination, although in making this point he runs through a door already opened by David Blackbourn, whose work is not properly acknowledged in this respect. Contributing one of his more occasional pieces, Jarausch is also reliably sensible and thorough. But the other essays are very slight, ranging from the inconsequential to the routine.

More to the point, the volume fails to deliver adequately on Remak's revisionist promise. Volumes such as this depend in any case for their coherence on the strength of the editorial vision, but here the claim that the editors are offering an alternative view of the *Kaiserreich* makes editorial vision all the more key. Unfortunately, the starting hypothesis (if one can even call it that) is banal: Remak wishes to find "what was being done well" in imperial Germany and "why so many Germans . . . were basically pleased with the society in which they found themselves" (p. x). He was puzzled by "why the tenor of Wilhelminian historiography had come to be so overwhelmingly negative" and so embarked on a "quest for the non-perverse in Wilhelminian Germany" (pp. ix, x). This might have been the starting point for some thoughtful and agenda-setting critiques of the field. As indicated above, Beyerchen does this, and in some of the weaker essays there are glimmers of such a forward-moving discussion, as in Showalter's idea of the army as a nationally integrative institution or Lees's treatment of an affirmative urban culture as against the influential stereotype of anti-urban "cultural despair." But in none of these cases is there any effort to conceptualize the basis for a more constructive comparative perspective. More damning still, there is no hint of explicit engagement with existing revisionist critiques of the literature on the *Kaiserreich*, notably in relation to the continuing debates over the so-called Ger-

man *Sonderweg*. The discussion is all at the level of "things were much better in this particular area of life under the Empire than the critics allow" or "things were bad elsewhere in this respect, too." This is hardly the way to move our understanding forward, and Remak's ill-considered "optimism" will not persuade the adherents of the "pessimistic" reading of the *Kaiserreich's* stability and viability that they should change their minds.

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JOHN HIDEN. *The Baltic States and Weimar Ostpolitik*. New York: Cambridge University Press. 1987. Pp. xi, 276. \$39.50.

"A paradox, a paradox, a most ingenious paradox!" John Hiden might have used the line made famous by Gilbert and Sullivan as the theme of his study of German-Baltic relations in the years after World War I. He argues persuasively that successive governments of the Weimar Republic pursued a policy in the Baltic region that aimed at revising the limitations of the postwar settlement by promoting increasing stability and consolidation of the independence of the states of Estonia, Latvia, and Lithuania. Thus, the paradox: revisionism and stability went hand in hand.

In the confusion of the military collapse and continuing warfare in Eastern Europe after 1918, such a Baltic policy was initially characterized by "uncertainty and improvisation" (p. 12) but became progressively stronger and more coherent as the postwar settlement for Eastern Europe and Russia took shape. By August 1920 a constructive policy committed "to the cause of good Weimar-Baltic relations" (p. 34) had come to predominate in the German government. It remained, not without resistance and challenge, the dominant element shaping German-Baltic relations throughout the next decade.

The policy drew on what Hiden identifies as a "trilogy of interests" (p. x) for the German nation in the Baltic that linked foreign policy goals, economic self-interest, and protection of German minority status—but not to the detriment of the government's good relations with the Baltic states. By 1923, when Gustav Stresemann took over leadership of Germany's foreign policy, he had only to build on the foundations laid between 1919 and 1923: cultural and material support of the German minorities, especially in Latvia and Estonia; active encouragement of German economic collaboration, a topic Hiden covers extensively; and the increasingly systematic and effective use of the policy to check the extension of a Franco-Polish

alliance in the Baltic region. Such an alignment would have been hostile to both Germany and Russia and would have been less satisfactory economically for the Baltic states themselves than the collaboration that Germany offered (pp. 142–43).

Hiden sees all of this as illustrative of the "constantly evolving concept of revisionism in the Weimar Republic" (p. 152), whereby the German government came to realize that its contributions to the enhancement of stability in the Baltic—and elsewhere—could represent a positive step toward Germany's national interest in reasserting its status in the postwar world. Hiden judges this "consistent and patient effort made by German governments since 1919 to develop a basis of trust with the new Baltic governments" (p. 158), especially as it applied to the pursuit of mutual economic interests, to be "as interesting for the historian of Weimar foreign policy as the end which it purported to serve, namely to prevent the Baltic states from coming under Polish control" (p. 188).

The book explores a great deal more than this short review can sketch: trade negotiations, the illusion that the Baltic states could become an economic "bridge" to the rich Russian market, Germany's eschewal of force as a means of revisionism, the limiting factors on support for German minorities in the region.

The problems that one encounters in Hiden's presentation involve structural and technical issues rather than content. The various themes come together most effectively only very late in the book. Actors are mentioned with no adequate introduction or explanation and often without benefit of first names, as with, for example, Hentig (p. 105), Riesser (p. 146), and Riesser and Kopp in successive sentences (p. 147). In this age of word processing, it ought to be possible to run a search of names and identify or reidentify people adequately so that the narrative remains understandable to the nonexpert. On the other hand, the book is cleanly produced and free of typographical errors, a distinction worth noting in an era that sees too many sloppily printed volumes.

Hiden's book offers cogent support and well-documented data for an argument that he states clearly in his opening remarks: "Weimar foreign policy provided opportunities for advancing and defending national interests without automatically threatening the rest of Europe" (p. x).

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HANS-ULRICH WEHLER. *Entsorgung der deutschen Vergangenheit? Ein polemischer Essay zum "Historiker-*

streit." Munich: C. H. Beck. 1988. Pp. 248. DM 14.80.

Since 1945 virtually no one in Germany has dared to justify the Nazis, but there has been a deep split on one question. To what extent did German society share responsibility for their deeds? In the aftermath of the Nazi regime, such established historians as Friedrich Meinecke, Gerhard Ritter, and Hans Rothfels asserted that the Nazi period had to be understood as part of a European tradition of mass democracy with few links to the German past. Ritter and Rothfels saw the real opponents of the Nazis among the German conservatives within and outside the army. Germany's peculiar political development and its involvement in a series of wars resulted from geopolitical factors, its tragic *Mittellage* in the center of Europe surrounded by enemies. In the 1960s this sanitized view of modern German history was convincingly challenged by a number of younger historians, including Hans-Ulrich Wehler, who sought to investigate critical social, economic, and intellectual factors that contributed to the incomplete democratization of modern Germany.

Since the political *Wende* of 1982, it has become increasingly acceptable again to question German responsibility for the excesses of war and genocide. In this volume Wehler analyzes the main apologists who came to the forefront in 1986 in the *Historikerkstreit*. Wehler frankly describes his book as a polemical essay, a polemic justified because the controversy involves political issues much more than it does scholarly questions. He rejects the *historicist ideal of impartial objectivity* raised by Thomas Nipperdey in defense of the apologists and argues that every work of history involves a perspective that the historian should acknowledge while, at the same time, honestly confronting the evidence. He demonstrates that the historians whom he examines in this volume have failed to do this. Ernst Nolte, in maintaining that Hitler got his genocidal ideas from Stalin and used them in reaction to his fears of Marxism, completely overlooks the tradition of anti-Semitism in Austria and Germany from which Hitler obtained ideas and support. Nolte actually justifies the internment although not the destruction of Jews as potential enemies by Hitler. Andreas Hillgruber defends as honorable the struggle of the German armies on the eastern front in 1944 and 1945 to protect German civilians, although he admits that exterminations continued. The dismemberment of the German Reich and the expulsion of the Germans is seen by Hillgruber less as a response to Nazi atrocities than as a consequence of long-range Allied aims fueled by an unjustified Prussophobia. Michael Stürmer takes up again the

geopolitical thesis, also defended by Hillgruber, of the German *Mittellage*, which exculpates Germany.

For Wehler the attempt of the revisionists to "detoxify" (*entsorgen*) the German past is not merely an intellectual exercise but also one part of a conscious effort to legitimize conservative political and cultural policies. The sense of "national identity," which Stürmer and others wish to restore, signifies a return to the discredited values of the old Reich. Instead, Wehler argues, citing Jürgen Habermas, Germans can be proud of the democratic achievements of the Federal Republic, its opening to the political culture of the West, and its intellectual and cultural pluralism, which the new revisionists are challenging.

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OTTO DOV KULKA and PAUL R. MENDES-FLOHR, editors. *Judaism and Christianity under the Impact of National Socialism*. Jerusalem: Historical Society of Israel and The Zalman Shazar Center for Jewish History. 1987. Pp. 558.

This collection of twenty-nine essays is the result of an international symposium held in Jerusalem in 1982. The first attempt at a comprehensive account of relations between National Socialism and the churches, it is distinguished more frequently by interdisciplinary and synoptic qualities than by new findings or dramatic interpretations. The initial contributions on pre-Nazi anti-Semitism cover fairly familiar ground, although Paul R. Mendes-Flohr substantially enriches our understanding of the Weimar years by showing how persistent Christian prejudices against Jews and Judaism crippled initial attempts at Jewish-Christian dialogue. Continuities in the history of anti-Semitism are underlined by Shmuel Ettinger, who develops the controversial thesis that modern forms of Judeophobia are secularized versions of ancient religious enmities. Rudolf Lill's sharp distinction between the secular racism of the National Socialists and religious anti-Semitism is forcefully controverted by the late Hermann Greive, who stresses the ties that bound Christianity and Nazism together and that prevented Christianity from recognizing in Nazism the executioner of both Jews and Christians.

Essays on the churches in the Third Reich soften but do not reverse past criticisms of their failures to rebut anti-Semitism and defend Judaism. Konrad Reppen acknowledges that German Catholics were more concerned with their own tribulations than with those of the Jews, but he also disputes charges that they ignored official anti-

Semitism. Catholic denunciations of racial priorities in Nazi ideology and reaffirmations of the Jewish components of Christianity were necessarily indirect attacks on Judeophobia. Private protests by church leaders that saved some Jews in mixed marriages would have failed had they been accompanied by public protests against deportations. Repgen calls attention to clandestine aid to individual Jews, as does Richard Gutteridge in his rather more critical assessment of German Protestants under Hitler. Had Protestants and Catholics joined in denouncing the *Kristallnacht* pogroms of 1938, Gutteridge argues, the regime probably would have been forced to place limits on its anti-Jewish policies. The price, however, would almost certainly have been the destruction of the confessing church. Otto D. Kulka reiterates his view that knowledge of the Holocaust was commonplace in Germany by 1943, although it is not clear that the widespread circulation of rumors arising chiefly from *Einsatzgruppen* actions constitutes evidence of such knowledge.

The impression that aid and heroism came primarily from a small number of laity, lower clergy, and religious minorities, whereas lethargy and perhaps necessary compromise prevailed elsewhere in the churches, is deepened by separate essays on the countries of Nazi-dominated wartime Europe. Michael R. Marrus's essay on the French churches is a model analysis of the complex interplay of political and theological issues that determined the diverse responses of church authorities to anti-Jewish measures. On the whole, the articles on Eastern Europe are the least satisfactory and reflect the difficulty of gaining access to sources. Zygmunt Zielinski's examination of the rescue of Jews by Catholic orders in Poland is a useful addition to our knowledge but does little to explain why so few Jews survived as a result of aid from Poles. Several contributors point out that a major obstacle to high-level aid was Hitler's obsession with winning his war against the Jews. Meir Michaelis, convinced that Pius XII was deeply troubled by the plight of the Jews, maintains that an open Vatican condemnation of Nazi racial policies would have endangered the thousands of Italian Jews hidden by the church without otherwise altering the course of the Holocaust. The pope's concern is substantiated by John Conway, who draws on newly published Vatican documents to show that Pius XII considered diplomatic maneuvering his only means of saving Jewish lives. More generally, Conway makes an important point when he warns against overestimating the influence of religious institutions in an age of inflamed nationalism.

The concluding chapters explore post-Holocaust Christian-Jewish relations and especially

Christian attempts to reinterpret classical church texts in ways that reassert Christianity's roots in Judaism, remove anti-Semitic overlays, and recognize the authenticity of the Jewish faith. They also raise questions about how much of this can be achieved unless Christianity abandons claims of absolute and universal significance. If here and occasionally elsewhere moralistic and polemic tones are heard, they do not typify an anthology that maintains a high general level of scholarship.

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WINFRIED BECKER. *CDU und CSU 1945–1950: Vorläufer, Gründung und regionale Entwicklung bis zum Entstehen der CDU-Bundespartei*. (Studien zur politischen Bildung, number 13.) Mainz: v. Hase und Koehler. 1987. Pp. 510. DM 42.

This book of some three hundred pages of text, sixty pages of densely written notes, and another one hundred ten pages of documents deserves the adjective *materialreich*. Basing the account on extensive and exhaustive research in a wide variety of primary sources and secondary works, Winfried Becker narrates the early history of virtually every Christian Democratic group. The book is also *namenreich*; it seems that virtually all of the county and local activists of the Christian Democratic Union (CDU) are mentioned.

This part of the work, which makes up the bulk of the text, is undoubtedly the most useful section of the study. Researchers working in this period will be grateful to Becker for the wealth of information he provides on local politics in the immediate postwar period, particularly since admirably complete indexes of localities and names give ready access to the information.

The author's larger thesis, however, raises some questions. Becker intends to explain the significance of the CDU's early years in the context of German postwar political history, and he contends that the Christian Democrats were the only really new political force to emerge in Germany after 1945. Indeed, Becker claims that the CDU was the real answer to what he calls the failed ideas of the nineteenth century, which led to the totalitarianisms of the Left and Right in the twentieth century. The author asserts that the CDU was able to perform this function because of its firm ideological and moral grounding in the precepts of applied Christianity, including the traditions of Christian political activism (such as Adolf Stöcker's Christian Social movement and the Center party) and the anti-Nazi resistance.

Students of postwar West Germany will recognize Becker's interpretation as very similar to the

visions of Konrad Adenauer. Perhaps not altogether surprisingly in a book sponsored by the Konrad Adenauer Foundation, the Federal Republic's first chancellor is the real hero of the book. Becker argues that Adenauer laid the basis for the rise of the CDU and the future stability of West German democracy by recognizing as early as 1945 not only that the Christian Democrats had to be an interconfessional party to be successful but also that, because of "the hegemonial partition" of Europe into East and West, the party had no real future in the Soviet zone of occupation.

My primary criticism of the book is that it sees German politics after 1945 too much through the eyes of Adenauer. This manifests itself in small but jarring ways, no less than in matters of large-scale interpretation. Becker, like Adenauer, consistently brackets the Social Democratic party (SPD) and the Communist party as a single political entity, the "Marxists." And, although it is true that Adenauer thought of the German Democratic Republic in quotation marks, continuing this practice in a book published in 1987 seems unnecessary and offensive. More basically, was Adenauer really farsighted in his recognition of the effect of Soviet policies—the division of Germany and Europe—or was his decision actually a contributory cause to the effect both the chancellor and Becker lament?

The author's admiration of Adenauer also leads him to treat both the men who opposed the future chancellor (for example, Jakob Kaiser) and their ideas in an unduly one-dimensional manner, and Becker is strangely defensive about Adenauer's neoconservative and Red-baiting tendencies. Becker seems unaware of the irony that Georg Kiesinger, who served the Nazis in the propaganda office of the Reich foreign ministry until 1945, lectured the delegates at the CDU's 1950 national congress in Goslar on the path of totalitarianism from Rousseau to Lenin.

Becker argues that parliamentary democracy in the Federal Republic owed its success to the vision and politics of Adenauer, but in many ways the chancellor personified the old, not the new, politics. He never gave up his belief in political polarization. For Adenauer, there was no sense of a give and take between majority and opposition; for him (and for Becker) the Social Democrats remained the enemy and had to be excluded from power. In general, there is too little in this book about the continuity between what one of the CDU's founders (not Adenauer) called "the heroes of Weimar" and post-1945 German politics. Many of the early local and regional CDU leaders formed coalitions with the Social Democrats. Adenauer invariably opposed such cooperation, but these coalitions had a venerable tradition. They

went back to the long-term and fruitful arrangements of the Weimar parties in Prussia between 1918 and 1932. The Weimar coalition—which Adenauer also opposed—provided Prussia and by extension Germany with long years of political stability. For the future of the Federal Republic it was fortunate that Adenauer's (and Schumacher's, but that is another story) vision of the politics of polarization was not followed by his successors in the CDU.

But I do not want to end on a negative note. Despite these criticisms, Becker's work is an important and welcome book.

DIETRICH ORLOW
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GEORGE HOLMES. *Florence, Rome, and the Origins of the Renaissance*. New York: Clarendon Press of Oxford University Press. 1986. Pp. xiv, 273. \$45.00.

George Holmes's aim in this book is to synthesize central Italian political and cultural history from the 1260s to about 1320. Each of two parts on the periods before and after the mid-1290s begins with chapters on politics and economic matters and proceeds to more extended essays on painting, sculpture, and poetry, in order "to achieve . . . a picture of the influences which were important in creating a particular outburst of creativity of a revolutionary kind" and especially of the "imaginative inventiveness of two men" (p. 159)—Dante and Giotto—who deserve to be considered the "very first stage in the creation of Italian Renaissance culture" (p. 263). Holmes poses the question of influence in terms of how artists and writers responded to classical models and how regional politics gave them opportunity and incentive to look beyond their own localities. Before the transfer of the papal court to Avignon, Rome was the center from which political and cultural influences and the patronage of the popes molded the civilization of the Tuscan and Umbrian cities. Holmes's regional perspective is a useful warning against the tendency to treat the cultural history of these towns apart from the network of central Italian contacts and influences.

Permeating these essays is an old but still lively question: how far can art and literature be interpreted as emerging from social contexts? Holmes's choice of themes suggests a certain ambivalence on this point. Part 1 includes chapters on religion and lay thought (focusing on the Franciscans and the rhetorical-classical culture of the notaries), which serve as a bridge between the political and economic background and the painting, sculpture, and poetry of the late thirteenth century. Curi-

ously, part 2 lacks corresponding treatments of these more popular dimensions of culture for the next generation. Holmes seems less concerned here with relating political and social contexts to the aims and reception of culture than he is in treating the period before 1300. The overall impression is one of some uncertainty about just what we need to know (and why) about the politics and economics of the age in order to interpret its poetry and painting.

Holmes's general approach to communal society assumes that the politics that mattered was an affair of semifeudal elites of great families. It may be true that, "though the cities of thirteenth-century Tuscany derived much of their wealth and political importance from industry and trade, . . . this was a recent economic growth grafted onto an ancient system of social customs which retained its dominance over families and their rivalries" (p. 5). But we risk missing much of both the politics and the culture of these cities if, on this basis, we conclude—as Holmes does—that "political division within Florence was primarily a matter of family feud," that, "although the importance of class divisions was much emphasized by Florentine as by other city chroniclers, it is wrong to base on this characteristic an image of city politics divided by economic interest groups" (pp. 164–65), and that the issue of class structure in Florentine government is an "overworked problem" (p. 29). What we miss, among other things, is why family feuds and the attempts to control them were so controversial and what those trying to contain upper-class *prepotenza* hoped to put in its place. Much of this society's culture, including its painting and poetry, emerged from the urgency of this problem in the form of polemics, pragmatic inquiries, and philosophical reflections on the proper organization of society itself and on the alternatives to aristocratic hegemony. For the most part, these came not from the elites but from the literate and politically conscious *popolo* of merchants, guild members, notaries, lawyers, friars, poets, and artists-artisans. The political and social context of their interest in Aristotle and Cicero and in the ideals of good government and the attempts to depict them, of their complex fascination with the egalitarian notions of Franciscan poverty and the imitation of Christ, and of the pictures and texts generated by that fascination lies in the disputes over whether politics *ought* to have been, as it so often was, "primarily a matter of family feud" and whether and how it could be, as indeed it sometimes was, something very different. The dynamic link between politics and culture in the Italian communes was this pervasive concern with the nature and reform of social bonds, institutions, and power relations among classes. In

whatever form—whether partisan or theoretical, religious or secular, painted, sculpted, or written, prose or verse—its highly charged relevance to political conflicts within the cities escaped no one—and certainly not Dante.

JOHN M. NAJEMY
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ROBERTO BIZZOCCHI. *Chiesa e potere nella Toscana del Quattrocento*. (Annali dell'Istituto storico italo-germanico, number 6.) Bologna: Mulino. 1987. Pp. 412. L. 38,000.

Roberto Bizzocchi has written a detailed, readable, and exciting study of the relationships between the rulers of Florence and the Catholic church during the fifteenth century. This was the very period in which Florence extended its dominion over most of Tuscany while simultaneously transforming its government into a Medici-controlled oligarchy. Also in the fifteenth century the popes, reestablished in Rome, began the centralization of church administration and the rationalization of the papal state. The management of church property and personnel was a matter of serious concern to the rulers of Florence because the proportion of clerical population, wealth, and land in its dominion steadily increased, rising to about one-third of the whole by the sixteenth century. Increased papal intervention in the provision of benefices, in criminal matters, and particularly in fiscal affairs, where ecclesiastical and lay jurisdictions overlapped, brought about broad areas for church-state controversy.

Bizzocchi explores these relationships in depth using archiepiscopal, episcopal, communal, and papal archives. His findings are fascinating. The ecclesiastical establishment in Florence and its *condado* mirrored the lay establishment; members of the same families occupied both kinds of offices. Political fortunes and misfortunes of Florentine families were also reflected in their membership in church offices at all levels. The republic and later the Medici allowed local ruling families to dominate the bishoprics and cathedral chapters outside of Florence, with the exception of Pisa, and papal provisions of all manner of sacred offices usually followed Florentine wishes. Overlapping jurisdictions and tribunals seldom caused serious controversy, the secular and ecclesiastical spheres tending to interpenetrate and cooperate. Problems of taxation were more delicate, and Florentine magistrates throughout the century had to seek absolution for collecting moneys from church property without papal license. The relative ease with which such absolution was obtained, however, suggests

that even here Florence and Rome found their differences negotiable.

Indeed, negotiation was the key to Florentine success in gaining and keeping effective control over the church property and population within its territory. With a wealth of revealing illustrations from archival material, Bizzocchi demonstrates that Florentines used skillful, tactful, and above all persistent diplomacy in Rome. Even with formal relations in disarray following the Pazzi conspiracy of 1478, the spirit of compromise was not lost. Among the knotty problems discussed at a peace conference in Rome in 1481 was a tax of six thousand ducats that Florence had imposed on the clergy to facilitate the war against Pope Sixtus IV. Here, with the papal negotiator none other than the same Girolamo Riario who had provoked the conflict, a compromise was ultimately achieved. The pope granted Lorenzo de' Medici a license to collect twenty thousand ducats from the Tuscan church, one-third of which was to go to Rome for the crusade against the Turks. Then, with the election of Giambattista Cibo as Pope Innocent VIII and marriage negotiations between the Medici and Cibo families, Florentine interests and papal interests merged. The appointment of Lorenzo's son Giovanni as cardinal, and (with the cardinal's hat) the assimilation of the Medici among the princely families of Italy, brought the assurance that Florence would continue to dominate the church in the following century.

BARBARA MCCLUNG HALLMAN
California Polytechnic State University

SILVANA SEIDEL MENCHI. *Erasmus in Italy, 1520–1580*. Turin: Bollati Boringhieri. 1987. Pp. 530. L. 50,000.

For fifteen years and more, Silvana Seidel Menchi has been studying the influence of Erasmus's books on Italian Protestants or those who leaned in that direction. She has also located and studied numerous Italian printings and translations of Erasmus's works. This very substantial book is the result.

What emerges is an "Erasmus luterano" (p. 41), that is, an Erasmus who inspired many Italians to break with the papacy. Several themes inspired by Erasmus were particularly important to the development of Italian religious dissent. Readers initially applauded Erasmus's attacks on the hierarchy and abuses of the church. Italian followers of Erasmus proclaimed that the human being's dialogue with God was not bound to specific rites or works, because Christ had abolished once and for all petty religious requirements. Hence, Erasmus's

condemnation of church ceremonies, and the like, intersected with Luther's rejection of justification through works to produce what Italian reformers called "evangelical" or "Christian liberty," a kind of nonspecific Protestantism. Further, Erasmus's *De immensa Dei misericordia* (1524) inspired the notion of an "open heaven," meaning that all human beings could be saved. Mercy became synonymous with grace; belief in salvation produced salvation. In other words, it was an interpretation of Erasmus close to Luther's justification by faith but lacking Luther's overpowering sense of sin. A third Erasmian theme was critical doubt, that is, a radical questioning of ecclesiastical claims. Menchi does not claim that these themes are the sole way of interpreting Erasmus's works, or that they are necessarily correct, but that Italian dissenters derived such ideas from him.

Schoolteachers emerge as a chief means of spreading an Erasmian Reformation; they taught the *Colloquia* in class and more radical religious views to adults outside. In their wake followed artisans and middle-class men and women. Menchi also charts Erasmian influence through the rise and decline in the number of Italian printings of his works.

Along the way the author offers a number of succinct judgments on current scholarly issues. She rejects the notion of evangelism as an ambiguous, halfway position between Catholicism and Protestantism. Similarly, she does not see Erasmus as an ecumenical conciliator above the sectarian fray but as a strong polemicist and a sign of contradiction. Occasionally, one may disagree with some nuances of her argument, as in the question of influences. When Italian heretics appeared before a local inquisition, they might be found in possession of Erasmus's works, plus some by mainline Protestants such as Melancthon, and they might also confess associations with other heretics. From which source did they derive their Protestant views? Of course, influences normally flow together, and dividing them into separate streams is an artificial exercise. Menchi, however, inevitably insists on a dominant Erasmian influence.

The book is written with great vivacity, is massively documented, and almost entirely free of misprints. It is based on the most comprehensive investigation of Italian inquisitorial records yet undertaken, including her discovery of the previously unknown records of the Holy Office of Imola. This is a distinguished contribution to the history of the Protestant Reformation in Italy and the story of the *fortuna* of Erasmus. It should be translated as soon as possible.

PAUL F. GRENDLER
University of Toronto

GISELA FRAMKE. *Im Kampf um Südtirol: Ettore Tolomei (1865–1952) und das 'Archivio per l'Alto Adige'*. (Bibliothek des Deutschen Historischen Instituts in Rom, number 67.) Tübingen: Max Niemeyer. 1987. Pp. vii, 328. DM 84.

As I browsed through the detailed table of contents of this monograph, before tackling the text, I was reminded of a dictum by my colleague James Billington, now the Librarian of Congress. He has urged fellow historians and other humanists to worry less about finding definitive answers to unimportant questions and more about providing tentative answers to important questions. Was this, I asked myself, a scholarly work that treated exhaustively a topic of secondary importance? And did Ettore Tolomei, an intellectual of modest gifts and only regional reputation, merit a lengthy monograph?

I had not read very far into the text when I realized that my skepticism was unjustified. Gisela Framke's work is a fine example of how specialized monographic studies, at their best, can provide insight into very large questions of enduring historical significance. Among the important questions explored here is the role of intellectuals and propagandists in creating a climate of opinion in which scholarship becomes the handmaiden of politics.

Framke devotes a full chapter of her study to a description and discussion of the antecedents of Tolomei's *Archivio per l'Alto Adige*. This is very useful not only to our understanding of Tolomei's own cultural and political background but also to our understanding of the complexity of cultural and ethnic conflicts in the region prior to the outbreak of World War I. Indeed, a separate monograph might be written on the strategies employed by a declining Hapsburg government to hold things together.

The core of the monograph, however, is Framke's research on the founding of Tolomei's *Archivio* and on its political and cultural evolution in the interwar period. If the detailed discussion of the *Archivio*'s editorial policies becomes occasionally boring, Framke nevertheless provides interesting examples of how institutions and publications dating back to the turn of the century were turned gradually—usually by cooptation rather than coercion—into instruments of Fascist power plays and political goals. Tolomei, it seems, was a willing instrument of Fascist Italy's policy of "Italianization" of the Alto Adige/South Tyrol, though some of his collaborators were not. On the opposite side of the fence were Pan-German intellectuals and politicians who promoted the myth of a great "repatriation" of the German element to Bavaria or Austria. Framke does an excellent job

of explaining how the two groups of extremists—Italianizers and Pan-Germans—worked for compatible goals under the more or less watchful eyes of their respective governments. One of the ironies of the situation, Framke notes, was that both groups of local activists and both totalitarian governments built their propaganda and their ethnic policies on anthropological, linguistic, and historical falsehoods. For all of their fascination with a more or less mythical past, they were not willing to come to terms with the ancient roots of most of the region's people, which were neither Italian nor German but Ladino.

Last but not least, Framke raises important questions about the relationship of the Alto Adige/South Tyrol question to the larger context of European, particularly Italian, foreign policy. Among the most interesting pages in her book is her treatment of Tolomei's concept of "internal colonialism." In the view of the *Archivio*'s contributors and theorists, the struggle to transform (reclaim, as they saw it) the culture of the region was of a piece with Italy's efforts to establish a colonial empire. Only a truly national state, free of ethnic minorities, they thought, would find the spiritual stamina to colonize other lands.

CLARA M. LOVETT
George Mason University

JAMES C. DAVIS. *Rise from Want: A Peasant Family in the Machine Age*. Philadelphia: University of Pennsylvania Press. 1986. Pp. xv, 165. \$21.95.

James C. Davis's lucid and enjoyable volume traces the transition from serfdom to urban industrialism as reflected in the history of a Slovene family, the Žužeks, living on the fringes of the Mediterranean, Central European, and Balkan civilizations in the barren and infertile highlands above Trieste. As the author points out, although most of our ancestors experienced modernization in the distant past, in this peripheral area of Europe the process is recent enough so that we may reconstruct it from the viewpoint of individual experience. Drawing on interviews with members of the family (into which the author married while serving in the U.S. Army in Trieste at the close of World War II) as well as a variety of archival sources including castle charters, police reports, church registers, and tax records, Davis documents the great social, political, and economic upheavals that punctuated the period from the late sixteenth century through the 1970s as mirrored in the microcosm of provincial life. Thus, in contrast to many, perhaps most, accounts of the modernization process that have focused predominantly on large-scale formal political, economic,

and social institutions and the elites associated with them, this book underscores the feasibility of approaching the study of history and society through the medium of the individual or small group whose ideas and actions can be conceived of as diagnostic of the larger and more abstract order.

Davis's work straddles the boundary between history and ethnography, and its subtleties and inferences require some knowledge of both. Although many of the data are fragmentary, especially for the earlier periods, the author skillfully and, for the most part, convincingly extrapolates from them to create a vivid picture of village life encompassing four centuries from a time of feudal obligations and an impoverished subsistence agricultural economy to that of wage labor, entrepreneurial activity, and partial urbanization.

If the book has a major failing, it is that it is neither a complete historical survey of this remote corner of Europe nor the kind of integrated, holistic ethnographic account to which anthropologists are accustomed. Nevertheless, given the paucity of information available to the author about daily life from the premodern period, he has done an admirable job in teasing forth a great deal of detail from his limited sources. In this respect, Davis's most insightful descriptions and analyses deal with the relationship between such large-scale events as the end of serfdom, on the one hand, and changes in family structure, marriage and inheritance patterns, and village-level economic and reproductive behavior, on the other. Similarly, he demonstrates how the decline in infant mortality as a result of the introduction of public health measures and better nutrition has affected the course of family relations and the underlying values associated with them.

Although, even for relatively recent times, Davis has not been able to communicate more than a fragmentary picture of village life, he has nonetheless succeeded in conveying to the reader a genuine feeling for what it was like to be a Slovene peasant at various periods in history. In this regard, throughout the book the reader is asked, both explicitly and implicitly, to project from the specific and limited experience of the Žužeks the fate of millions of other such "obscure and illiterate people" (p. xiii). Perhaps most significantly, the author explains that he is writing not entirely for scholars interested in the subject but also for "all those who have, somewhere among their ancestors, a poor peasant or two" (p. xv). Indeed, for those of us who are both academics and recent descendants of such persons, this book holds a double significance. Finally, I would like to note that this monograph fills a void in the anthropological literature on Yugoslavia, where, with one

or two notable exceptions, the Slovenes have been largely ignored in favor of more exotic studies of such phenomena as the *zadruga* and tribal society among other South Slavs.

ANDREI SIMIĆ

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JEAN-PAUL BLEDE. *François-Joseph*. Paris: Fayard. 1987. Pp. 766.

A new biography of Franz Joseph is not likely to abound in surprises, but one welcomes an appraisal that shows a command of the variety of sources available. Jean-Paul Bled admirably balances domestic, external, and financial affairs, and his shrewd psychological dissection of the emperor's relations with mother, wife, son, and Archduke Maximilian, his brother, will divert the general reader from the intricacies of politics at home and abroad. Even so, in those last two categories, the author is masterful in demonstrating his talents at condensation and lucidity. One is not overwhelmed by another exhausting excursion into the Crimean War, the negotiations for the *Ausgleich*, or the Bosnian crises of 1908–09.

Bled quotes with some disapproval (p. 702) Ernst von Koerber's sally that the last important Habsburg disserved the monarchy with, first, his youth and, last, with his extended old age. This study documents again the young ruler's vindictiveness in dealing with the rebel Hungarians in 1849, his ready adoption of his mother's absolutist fixations, and his extremely ill-advised assumption of the role of commander-in-chief in 1859. Still obdurate in his claim to primacy in the Germanies in the early 1860s, Franz Joseph showed a growing sense of accommodation after losing that struggle. To the Magyars in 1867 he conceded much, but not his ultimate control of the army and of foreign affairs. He endorsed, though he did not enjoy, collaboration with the German liberals in Austria for most of the 1867–79 period, but he was happier when Eduard von Taaffe brought back the more moderate Czech elements to parliamentary activity. During the years from 1905 to 1907, he pushed his ministers-president toward sweeping electoral reform in Austria to revivify what he ultimately detested, representative government.

Bled rightly exculpates Franz Joseph from the charge of immobility. As the years passed, he learned to cope with forces he could not possibly applaud. Despite all his efforts, he failed with his mentally troubled consort, whose fetishes and psychoses only a superbly patient husband could endure. The story of his late and platonic friendship with Katharina Schratt is part of Vienna's folklore. It is, then, something of an irony to learn

of his purely physical affair with Anna Nahowsky, which lasted no less than a dozen years after he met her in the park at Schönbrunn in 1875. Bled has no doubt that Empress Elisabeth had no such "extra dances," no matter how gallant the Magyar or English lords might have been.

The author's doctoral thesis at the Sorbonne in 1982 was *Les fondements du conservatisme autrichien 1859–1879*, and this may account for his very sensible delineation of the emperor's way with his chief ministers and advisers. He undoubtedly was in Felix Schwarzenberg's thrall in the earliest year of his reign, and he probably honestly felt close to Taaffe. When Taaffe failed, however, he met with a minimum of thanks in 1893. The man who sat for hours poring over dossiers and receiving thousands in audience could not allow sentiment to stand in the way of trying to rule an unusually complex empire.

On very rare occasions, the author raises questions of what might have been. Did his subject miss a historic chance, between 1905 and 1907, to effect a restructuring of the empire by forcing universal suffrage in Hungary? Bled replies that Franz Joseph was quite satisfied with dualism. More, as an aristocrat imbued with the notions of chivalry and feudalism, he believed he should keep his word. It is also quite likely that a man approaching eighty had a highly realistic understanding of what such an action from above would engender. Would he have believed, as Bled seems to (p. 640), that national passions were beginning to lose their intensity on the eve of World War I?

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MARIO D. FENYO. *Literature and Political Change: Budapest, 1908–1918*. (Transactions of the American Philosophical Society, volume 77, number 6.) Philadelphia: The Society. 1987. Pp. 156. \$20.00.

Although early modernism has been primarily identified with France, "historians of ideas," as Mario D. Fenyo says in his introduction, "have gradually come to realize the significance of Vienna" (p. 1), the home of Sigmund Freud, Ludwig Wittgenstein, and Robert Musil. The Hungarian *fin de siècle* was even more muddled than the Viennese; the desire for independence from Austria led to a patriotic, Romantic stance, inhibiting any true flowering of modernism. Hungarian intellectuals felt overpowered by Austria; while Austrian intellectuals became international celebrities, Hungarians had to keep reminding the world that some well-known figure in art or science was their compatriot.

Fenyo's study demonstrates how the journal

Nyugat (Occident) introduced modernism in the arts by uniting the most exciting minds in the country. His purpose is to show the political function of literature—a legitimate approach to Central Europe, where literature has often substituted for ideology. Fenyo also gives a succinct survey of the economic context of *belles lettres*, tracing the financial history of the founding and first decade of *Nyugat*. His objective treatment of the journal's relationship to the establishment is commendable; however, he overemphasizes its working-class contacts.

Examining the political attitudes of the contributors to *Nyugat*, he soberly contends that, in the wake of World War I, some of them temporarily abandoned the periodical's liberal, supranationalist stand. Fenyo offers an excellent summary of *Nyugat's* importance, suitable for those English-speaking students of Central Europe who do not know much about Hungarian intellectual life. An attractive volume on good paper with fine illustrations, the book is unfortunately marred by several easily avoidable typographical errors.

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THOMAS M. PRYMAK. *Mykhailo Hrushevsky: The Politics of National Culture*. (University of Toronto Ukrainian Studies, number 3.) Buffalo, N.Y.: University of Toronto Press. 1987. Pp. 323.

Mykhailo Hrushevsky (1866–1934) was perhaps the chief figure in the development of the Ukrainian national movement in the early decades of this century. A prolific historian and literary critic, he produced 163 published titles, including a ten-volume history of the Ukraine and a five-volume history of Ukrainian literature. He played a paramount role in the tangled events of 1917 and 1918. In June 1917, even as the Russian Provisional Government was launching a major offensive against the Germans, the Ukrainian Rada (Council) under his leadership declared the autonomy of the Ukraine; after the October Revolution, it created its own People's Republic, in which Hrushevsky served as the first president. His government negotiated a separate peace with the Central Powers, signed at Brest-Litovsk in March 1918. But dissension within the Ukrainian leadership and the Bolshevik successes led Hrushevsky to flee the Ukraine in 1919. He returned in 1924. He hoped to achieve an accommodation between Ukrainian national aspirations and the still-new Soviet system. The Soviets for several years allowed a wide range of Ukrainian cultural activities, but policies changed with the consolida-

tion of Stalin's power. Hrushevsky was arrested for interrogation in 1932. Although never tried and sentenced, he spent the last years of his life under surveillance and died as a result of surgery in 1934. Still, he was given a state funeral and a monument was erected to his memory in Kiev; on the other hand, official Soviet scholarship has until very recently dismissed him as a "Ukrainian bourgeois nationalist."

Thomas M. Prymak offers a sympathetic though not hagiographic biography of this important and clearly controversial scholar and statesman. He has carefully combed all of the archives accessible to him, conducted oral interviews with persons who knew the scholar, and read widely in the great mass of Hrushevsky's own writings. Prymak cites the many and bitter criticisms that Hrushevsky's policies and behavior have evoked even within Ukrainian nationalist circles. He nonetheless seeks to render the questionable political choices Hrushevsky made at least understandable, if not laudable. Hrushevsky emerges as a Romantic *narodnyk* (populist), who fervently believed that support from the masses would compensate for a lack of political organization. In 1907 he declared, "Ukraina farà da se [Ukraine shall make itself]." Although not without political skills, he naively abolished private property in a single grand gesture, which cost him support, and disbanded the Ukrainian regiments, which might have given his government critical military support.

Any biography of this controversial figure would be sure to evoke some disagreement among its readers. Even the Soviets both vilified Hrushevsky and allowed a monument to be raised to him. Prymak's accomplishment is rather this: he has written a superbly researched and lucid biography of a principal figure in modern Ukrainian history. And anyone interested in national movements within the Soviet Union today would be instructed by it.

PATRICIA HERLIHY
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ROBERT O. CRUMMEY. *The Formation of Muscovy, 1304–1613*. (Longman History of Russia.) New York: Longman. 1987. Pp. xv, 275. \$15.95.

In 1300 the East Slavic (future Russian, Ukrainian, and Belorussian) lands were backwaters of civilization, and in 1600 they still were, so their history of that time tends to be of more parochial than general interest. Nevertheless, the East Slavs were not totally isolated from the mainstreams of Eurasian economic, cultural, and political life but were, rather, on the periphery of several zones: Baltic, East-Central European, Balkan-Black Sea,

Caspian Sea–Persian, and Inner Asian. Russia's wax supplied many non-Russian candlemakers, its furs and hides clothed many Europeans and Middle Easterners, and Russia played a vital role in the world of Eastern Orthodox Christianity.

Backwater or not, the East Slavic lands experienced significant "development" and expansion. At the end of 1304, the Mongol Golden Horde controlled most of what came to be "European Russia"; a culturally predominant East Slavic Lithuanian Grand Principality ruled most of Belorussia; and Moscow, under Alexander Nevsky's grandson Yuri Danilovich, was one of several competing principalities for leadership of the Russian "north" under the Horde's suzerainty. By 1598, when Nevsky's line died out, the only significant Mongol remnant of the Horde was the Crimean Khanate, an Ottoman vassal. Meanwhile, Lithuania had expanded into most of the Ukraine but in 1569 ceded it over to the economically and culturally more advanced Polish partner of the dynastic union forged in 1386. At the same time Moscow had become a *tsarstvo* with its own patriarch and canon foundries and controlled a vast stretch of territory from the Gulf of Finland to Central Siberia, from the White Sea to the Caspian.

Robert O. Crumme's balanced and eloquently written volume, constituting the third in the eight-part "Longman History of Russia," is an excellent new synthesis of these three centuries that happily incorporates much of the recent Soviet and Western scholarship. Choosing, as do most historians, to concentrate on Muscovy at the expense of the western territories, he devotes five of his eight chapters to political history and one each to "land and people," the church, and culture. He brings to the work his own deep research and understanding of Russian religion and the Muscovite aristocracy, which dominated state and society without ever achieving the rights of the European nobility. His sections on Orthodoxy, the arts (with ample background from the eleventh century through the thirteenth), Ivan IV (the Terrible), and the Time of Troubles (1598–1613) are among the most successful brief treatments of these subjects that I have seen. On the other hand, given restrictions of space, some topics, such as technology, taxation, law, and education, are sacrificed, receiving little or no attention. The book has seventeen superbly explained plates in black and white, seven useful little maps, a fine bibliography (with a few gaps), but, sadly, no genealogical tables.

The author avoids grand schemes for historical development, Russian or regional, and boldly (too boldly?) presents Moscow's ascent in the fourteenth century and its recovery in the Time of Troubles as fortuitous ("against all odds"). Still he

sees analogies between the state-building policies of Ivan III, Vasili III, and Ivan IV on the basis of inherited medieval structures and the work of contemporary West European monarchs. Comparisons to other "Eastern" regimes and an analysis of Mongol influence on Muscovite institutions, however, are lacking.

Many of Crummey's points will be of use to professional historians. The divisive narrowness of several Muscovite churchmen is underscored. The patriotic Kulikovo cycle glorifying Dmitry Donskoi is correctly placed circa 1450, not in the 1380s. The Judaism of the "Judaizers" and the existence of a synod concerning monastic landholding in 1503 are treated with proper skepticism. Philotheus's (Filofei's) epistle with its classical formulation of the "Third Rome" concept is well analyzed. Rituals are given their due attention.

To serve the generalist and the specialist and to be useful in the college classroom—what more could one ask of the author?

DAVID GOLDFRANK
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RICHARD G. ROBBINS, JR. *The Tsar's Viceroys: Russian Provincial Governors in the Last Years of the Empire*. Ithaca, N.Y.: Cornell University Press. 1987. Pp. xiv, 272. \$32.95.

During the past two decades, historians of tsarist Russia have devoted increasing attention to the state bureaucracy. Most studies, though, have focused either on officialdom in the capital or on the implementation of individual policies such as the Stolypin agrarian reform. Richard G. Robbins's work on Russian governors takes us away from the Saint Petersburg world of high politics to the mundane sphere of provincial administration. In the process Robbins significantly enriches our understanding of the way in which tsarist Russia was actually governed.

The study, which concentrates on the period between 1880 and 1914, is essentially organized as a series of concentric circles with the empire's fifty governors at the center. Robbins starts by evaluating the training and selection processes for these viceroys of the tsar and describing their daily service routine. He then proceeds outward to examine the governors' interactions with their own staff, the personnel of other ministries, and the institutions of self-government (nobles' assemblies, zemstvos, and municipal dumas). The closing chapter of the book is devoted to the governors' dealings with the public at large, including particularly disorderly peasants and workers. Robbins uses this last section to indicate changes that undermined the traditional style of gubernatorial

rule, thus deftly returning the reader to the concerns with which the study began.

Traditionally, Russian governors have been viewed as powerful royal viceroys, chosen as favorites of the court, who often used their authority in arbitrary and corrupt ways. Robbins offers a revised and much subtler interpretation. First, he demonstrates that selection of governors became increasingly "professionalized" in the sense that candidates were expected to have followed a career pattern that gave them extensive experience in the provinces. More important, Robbins argues that, although some governors acted in the traditional viceregal fashion, most played a fundamentally different role. Social origin as noble landowners undoubtedly often affected gubernatorial performance, making the officials harsh in their treatment of rebellious peasants yet strangely sympathetic to the plight of striking industrial workers. So, too, could a governor's individual political views guide his actions. But the crucial determinant of the officials' behavior was the imperative of holding together a highly fragmented and poorly institutionalized local administration. Tact and compromise, rather than power and will, were the key prerequisites of success. In Robbins's terms, governors were less "satraps" than "supplicants" or "salesmen" (p. 243).

The picture of tsarist local government that emerges from the book is one of an administrative apparatus that worked adequately under routine conditions but rested on archaic foundations that were ill suited for the challenges of the twentieth century. Individual agencies, like the governor's chancellery, functioned well enough. Relations among institutions, including the supposedly contentious zemstvos, were normally harmonious. Yet surface calm covered deep structural shortcomings. State agencies at the grass roots were poorly staffed and badly coordinated. Even more important, the government was unable or unwilling to combine bureaucratic control with effective public participation.

One result of these shortcomings was the exaggeration of the personal role of the governor. The absence of clear rules for institutional interaction made the governor a go-between. The lack of supervision over officials by the courts or self-government meant that the governors had the hopeless task of policing all provincial functionaries, and the failure to involve the populace in state affairs compelled them to court public favor. The inadequacies of this personal solution for institutional problems was fully revealed in the crisis of 1905.

The nature of this study, a composite covering most of the empire's provinces for more than thirty years, imposes limitations. Change over time

is not always fully registered. Individual institutional relationships, such as the intriguing tension between governors and municipal dumas indicated in the book, are sometimes only partially drawn. Concluding comparisons with French and Soviet experiences are highly suggestive but briefly stated. The author, like his governors, must hurry on to the next task. Yet the overall strategy of employing the governorship as an avenue into the provinces is effective. Robbins has successfully combed disparate archival materials, memoirs, and secondary works to produce a richly crafted, persuasive analysis of a key aspect of Russian government.

NEIL B. WEISSMAN
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ROBERT EDELMAN. *Proletarian Peasants: The Revolution of 1905 in Russia's Southwest*. Ithaca: Cornell University Press. 1987. Pp. xv, 195. Cloth \$27.50, paper \$9.95.

A century after Russian Marxists and populists first debated the possibilities of peasant radicalism, historians are still puzzling over the nature of peasant activism under the old regime. Those in the Marxist tradition have sought evidence of class differentiation and class conflict within the peasant community as signs that capitalism had already begun to alter economic and social relations in the countryside, while those of the so-called culturalist school deny the emergence of antagonistic social strata and stress the enduring cohesiveness of peasant society. Robert Edelman's essay on the peasants of the right-bank Ukraine during the 1905 revolution addresses the theoretical and political issues involved in this debate and offers to resolve its seemingly intractable oppositions.

In the right-bank Ukraine, Edelman argues, modern productive relations combined with traditional customs to produce a hybrid form of peasant protest. While the local gentry engaged in large-scale commercial beet farming, the peasants themselves tilled the land in old-fashioned ways and lived in repartitional communes. They also worked as paid agricultural laborers on the beet plantations. In 1905 they struck for higher wages and better conditions, behaving more like factory workers than like peasants in other areas, who seized grain, timber, and land. Yes, Edelman notes, local strike leadership derived from the same intact communal structures, particularly the village gathering, that elsewhere guided customary forms of unrest.

Were these agrarian strikers the peasant proletariat of which Lenin dreamed? Yes, Edelman asserts, insofar as their revolt was motivated and

shaped by their position as wage-laborers. No, he says, in the sense that the landless neither led nor dominated the movement. Neither did the poorest villagers, so impoverished were the majority of peasants and so cohesively did the community react. The landless peasant wage-laborer, true analogue of the urban proletariat, remained marginal to village life and therefore to the decision-making process. The hero of the moment was instead the "semi-proletarian" (p. 132), the man (and often woman, Edelman notes) still attached to the land and to village ways but driven to protest by modern productive relations and to adopt methods appropriate to the nonpeasant world.

Edelman's insistence on the diversity and changing character of peasant experience is welcome (and much of his evidence is archival), but the terms of his argument are open to question. First, in distinguishing the two types of peasant behavior, Edelman stresses the contrast between impulse and reason as manifestations of traditional and modern frames of mind. He admits, however, that traditional tactics had a logic of their own; that instigators even in central Russia tended to be young, literate men (hardly exemplars of the old culture); and that the peasants of the right-bank Ukraine reverted to old-style tactics in 1917, when employers had disappeared from the scene. One might conclude that peasants everywhere tailored their responses to the nature of the target and arson and pillage were no more irrational (though more violent) than strikes. Second, Edelman cannot decide to what extent the example of working-class radicalism influenced peasant behavior (both in timing and form) or how "organized" that behavior actually was. Third, he claims that mobilization altered the traditional basis of peasant life, as village leaders lost authority and patriarchy was challenged by women's active role. But he notes that patriarchy and the commune survived even here.

Such ambiguities suggest that peasant strikers were perhaps no more proletarian than the pharmacy clerks or bakers who self-consciously adopted proletarian tactics and rhetoric in 1905. It is certainly worthwhile to use the insights of both schools of peasant analysis, as Edelman does, but the terms of the old debate may not deserve to survive.

LAURA ENGELSTEIN
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LOUISE ERWIN HEENAN. *Russian Democracy's Fatal Blunder: The Summer Offensive of 1917*. New York: Praeger of Greenwood Press. 1987. Pp. xv, 188. \$37.95.

Although based almost entirely on published sources, this concise study adds to our appreciation of a key episode in the Russian revolution. Louise Erwin Heenan leaves her readers in no doubt where she stands, as the title suggests ("gamble" might have been a more apposite term than the value-laden "blunder"). Her argument is that the ill-fated "Kerensky offensive" on the southwestern front in June 1917 irrevocably demoralized the troops, undermining the Provisional Government, along with its moderate socialist supporters, and facilitating the Bolsheviks' advent to power. The offensive mobilized the disillusioned right and center groups behind General Lavr Kornilov and destroyed the last shreds of Russia's credit with the Western allies. Heenan is convinced that there was an alternative policy: "Russia would have been wise to imitate her French ally and stay on the defensive until the Americans came" (p. 49). After all, the people clearly wanted peace; had their leaders responded to their needs, "perhaps Kerensky would have been right" when in 1931 he told a questioner, "we should be in Moscow now" (p. 129).

Attractive as this argument seems at first glance, it smacks too much of hindsight and begs too many questions to be wholly convincing. Heenan does not consider the likely American reaction to a Russian policy of total military passivity. How many more German divisions could General von Hindenburg have moved to the Western front than he eventually did before the arrival of U.S. troops? Was not Russian morale fatally undermined even before the June offensive? Heenan thinks not, contending that earlier that month, thanks to Alexander Kerensky's speechmaking and the committee system, "we see an army that is perhaps slowly stabilizing" (p. 106). But the telltale word here is "perhaps."

Her analysis of these committees' varied functions is, however, excellent, and she shows that the generals, as well as the politicians, were divided as to the merits of the proposed campaign. She also offers the first coherent narrative account of the inter-Allied conferences at which the fateful decision was taken to launch the offensive—originally scheduled for February 1917 but thereafter repeatedly postponed. She emphasizes the extent of Russia's sacrifices in the common cause, the reasonableness of its negotiators' demands, and the chilly response they elicited from Russia's Western partners. Yet in his well-documented *Strategy and Supply: the Anglo-Russian Alliance, 1914–1917* (1984)—which is not cited here—Keith Neilson has shown that Russian expectations were unrealistic, given Britain's dependence on the United States for funds and war materials. There was not enough shipping for the goods assigned, and

Russian ports were already choked with untransportable hardware. Heenan sidesteps or brushes off such mundane considerations, evidently feeling that the common soldier's desperate plight should settle all the arguments.

The offensive itself is handled rather cursorily (pp. 109–24). There is no mention of the significant air operations (over four thousand sorties in two months; twenty-three enemy planes shot down), and the map on page 110 is perfunctory, omitting the railroad lines along whose axes the Russians advanced. No works in German are cited, so that the Central Powers' counterblow, which won them control of eastern Galicia and Bukovina, receives short shrift. Nor does Heenan consider the suggestion by Franz Fischer (whose work has been translated) that the offensive's initial success helped bring on the German governmental crisis and the Reichstag's (defective) resolution favoring a compromise peace.

Despite these lacunae, Heenan deserves commendation for a skillful and sympathetic evocation of the imperial Russian army's efforts to maintain its viability in conditions that presaged disaster. Few readers will remain unpersuaded that its last offensive, launched largely for political reasons, was doomed to fail.

JOHN KEEP
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ALAN M. BALL. *Russia's Last Capitalists: The Nepmen, 1921–1929*. Berkeley and Los Angeles: University of California Press. 1987. Pp. xvii, 226.

Alan M. Ball's study of the era in Soviet history of the New Economic Policy (NEP) appears at an unusually timely moment. Cooperative and individual enterprise has been legalized and leasing is to be recognized as a coequal form with state property in agriculture and industry. Central to Mikhail Gorbachev's economic reforms has been the determination to incorporate commercial competition into an economy that will continue to be dominated by state ownership of the major means of production. NEP, not surprisingly, has returned as a subject of acute interest to contemporary Soviet policy makers.

For a phase representing the most obvious alternative to the Stalinist system of economic planning and administration, NEP has drawn surprisingly little scholarly attention in this country. Until now no one has studied closely its most distinctive social formation, the so-called *nepmen*, referring to the tradesmen and manufacturers who took advantage of the tenuous legal opportunities for production and trade during the 1920s. Moreover, the prevalence of illicit and semilicite com-

merce in the "shadow economy" in the Soviet Union both before and since NEP gives credence to Ball's belief that NEP legalized a marketplace that already existed under War Communism and that the suppression of NEP tended to drive petty commerce underground rather than mobilizing all productive energies into state channels.

The problem of the dualism, or failure of integration, between the state (including cooperative) sector and the private sector under NEP represented the stubbornly persistent outcome of the Soviet government's wavering and ambivalent policies toward the legal market. Ball shows that, although the logic of NEP required the state to open manufacturing, finance, and other branches to private entrepreneurship early in the NEP period, a phase of repression quickly followed, lasting from the end of 1923 until early in 1925. Legal private commerce reached a new peak in 1925–26 before succumbing to a longer, and more consistent, phase of disfavor from 1926 and 1927 to the beginning of Stalin's era of Five-Year Plans, when nearly all vestiges of NEP were swept aside by the great campaigns of collectivization and industrialization.

Under these circumstances, therefore, two hardy traits of NEP defined its character: it was built on the social and economic foundations that were bequeathed to Soviet Russia, though in reduced form, from the *ancien régime*; and the enterprising souls who took up the entrepreneurial challenge, risking exposure to the changing winds of state favor and popular suspicion, tended to tread lightly in their activity, concentrating mainly on buying and reselling foodstuffs and manufactures, generally not even from fixed commercial locations, rather than sinking investment into manufacturing. In short, NEP re-created, albeit at a still more primitive level, the sector of small-scale crafts, trades, and services that had been prevalent in the "other Russia" of folk society before the revolution. And the class of commercial entrepreneurs who rose to make use of the new market freedoms worked the interstices of the economy rather than its central productive units. They were, as a result, more susceptible to earning the opprobrium that attached to "speculators" both in Bolshevik party opinion and in the nepmen's popular reputation.

The Bolshevik Right counted on commercial competition between the state and private sectors to lead to their integration under a securely socialist framework and to an increase in the efficiency and productivity of both. Instead, partly because NEP largely re-created the existing dualism between the national market and the small-scale entrepreneurship of craftsmen and traders characteristic of the predominantly peasant society and

because of the Bolsheviks' inconsistent and often hostile attitude toward the private sector, nepmen sought as much economic independence from the state as possible. Although many bought from and sold to the state, Ball shows that, in response to state pressure, many moved their operations into the countryside, where they were freer of state licensing, taxing, and regulating controls, and began concentrating more and more on buying from and selling to private citizens, forming private enclaves outside the state's jurisdiction. Although these trends frustrated state expectations, they were in large part the consequence of the state's own unwillingness to grant nepmen the freedom and confidence that would have enabled them to increase the scale and visibility of their operations.

Indeed, Ball's book increases one's pessimism about the chances for success of the current reform efforts underway in Gorbachev's Russia. The forces supporting NEP were so fragile, and the popular antipathy to the nepmen so strong, that the divisions within the state over the proper degree of freedom to be granted to the private sector could not long remain resolved in favor of the Bukharinist Right. By the same token, the nepmen, as Ball shows, clung to the limited opportunity NEP gave them and never risked much of their own capital, rightly fearing that NEP would be short-lived. Much of their behavior, therefore, confirmed all of the worst fears of NEP's many adversaries: that it simply enabled a class of unproductive wheeler-dealers to profit from the many imbalances in Russia's economy and did not, on the whole, raise the level of the economy's productive forces. Still, as Ball makes unmistakably clear, the nepmen filled a necessary role in providing the citizenry in both town and countryside with a large proportion of basic foodstuffs and manufactures. The book's last sentence suggests that the party since NEP has reluctantly accepted the coexistence of the second economy with the first, a condition Ball likens to nepmen without NEP. Under Gorbachev, it might be more appropriate to refer to a policy of restoring NEP without nepmen. Yet Ball's book indicates how improbable it is that the state can sponsor cooperative activity as an intermediate sector between the legal private and legal state spheres without also giving rise to the stratum of energetic, mobile commercial entrepreneurs who give the marketplace its reputation as a haven for the grasping, individualistic, anti-Soviet petty bourgeoisie.

Ball's book provides much valuable evidence on these matters. The gap it fills is a large one. May this book stimulate further research on topics such as the cooperatives, state industry, and urban-rural relations under NEP, providing more knowl-

edge about that anomalous intermission between acts 1 and 2 of the Soviet revolution.

THOMAS F. REMINGTON
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ANN TODD BAUM. *Komsomol Participation in the Soviet First Five-Year Plan*. New York: St. Martin's. 1987. Pp. 62. \$25.00.

Although I share Ann Todd Baum's interest in the Communist Youth League, I cannot understand how she, evidently while still a doctoral student, could have been persuaded to rush this study into publication or how St. Martin's Press could have received a favorable evaluation of the manuscript from any well-qualified scholar. She does deserve credit for a good topic and for using, among other publications, *Soviet Youth: Twelve Komsomol Histories* (1959), edited by Nikolai Novak-Deker, a volume too often neglected, even by specialists. But, if she found that study, how could she have failed to use so many others? Her book, I would guess, draws on less than a tenth of the material already published that should have been consulted in order to give this topic solid treatment.

In addition to being incomplete, the book is marred by quite a few errors, large and small, including slips in transliteration and translation. And, beyond that, the book puts forward an unconvincing interpretation of the nature and role of the Komsomol during the First Five-Year-Plan, attributing to the league far more autonomy than is suggested by the evidence.

This is a work, then, that might have been praised as a student paper but should not have been shared with the public without a great deal more research and reflection. I hope the author will not let my words discourage her from exploring this topic further in the future.

RALPH T. FISHER, JR.
University of Illinois

NEAR EAST

JACOB LASSNER. *Islamic Revolution and Historical Memory: An Inquiry into the Art of 'Abbāsīd Apologetics*. (American Oriental Series, number 66.) New Haven: American Oriental Society. 1986. Pp. xv, 156. \$19.95.

Descendants of Muhammad's uncle al-Abbas first assumed the caliphate in 749 A.D. The last Abbasid caliph died in 1517 A.D., nine years shy of eight centuries later reckoning by Muslim lunar years. Only the first two of those centuries come in for detailed scholarly attention, however, because after that the caliphs enjoyed little real power and

were, more often than not, under the control of an amir or sultan who greatly restricted their activities. Only the Japanese imperial family can parallel this remarkably long-lived combination of political weakness and dynastic legitimacy.

The legitimacy and religious preeminence of the Abbasid caliphs are self-evident. Scholars differ greatly, however, in their appreciations of the influence of the caliphate. Buyid warlords from northern Iran, who as Shi'is believed in the primacy of the family of Ali, the son of another of Muhammad's uncles, instead of the family of al-Abbas, retained an Abbasid in the caliphate for a century after seizing control of Baghdad in 945. A little over a century before that an Abbasid caliph had arranged for the succession to the caliphate to pass to the Alids and then changed his mind. These and other instances raise the question whether the Abbasid caliphs were legitimate as symbolic leaders of Sunni Islam or as a family with ancient dynastic claims to popular loyalty.

To the extent that Abbasid legitimacy was dynastic, it was a triumph of their own myth making. Nothing in the Quran or the life of the early community after the Prophet's death pointed to his cousin Abdallah, the son of al-Abbas, as the progenitor of a uniquely legitimate dynasty. But Abbasid-period historiography decidedly does, as this well-conceived monograph by Jacob Lassner proves.

Lassner has long studied the subtleties of propaganda and purposeful distortion in chronicles of the Abbasid period. A previous book, *The Shaping of 'Abbāsīd Rule* (1980), developed a method of scrutinizing the chronicles for signs of tendentiousness that would have been appreciated by savvy Abbasid courtiers but are obscure to us today. His topic then, the legitimacy of an important Abbasid sublineage, skirted the main issues that other scholars of early Abbasid history have debated.

Now, his technique refined and decidedly more persuasive, Lassner has tackled the much more fundamental issue of the revolutionary myth that was generated to support the dynasty as a whole. He concentrates on the two generations of secret Abbasid leadership preceding the overthrow of the Umayyads and on Abu Muslim, the Abbasids' powerful agent. Lassner's gleanings from the chronicles are fascinating and bring into question many previously held assumptions. Although one may disagree with some of his conclusions—the chapter on Abu Muslim seems less convincing—they are always challenging and enlightening.

Whether Lassner's method of close reading will yield equally interesting results when applied to accounts of later periods remains to be seen, but he has unquestionably made an important contri-

bution to early Abbasid history. No student of the period can afford not to read this book.

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BENJAMIN BRAUDE and BERNARD LEWIS, editors. *Christians and Jews in the Ottoman Empire: The Functioning of a Plural Society*. Volume 1, *The Central Lands*; volume 2, *The Arabic-Speaking Lands*. New York: Holmes and Meier. 1982. Pp. 449; ix, 248. \$94.00 the set.

The Ottoman empire constituted one of the most diverse societies the world has ever known. Added to the native Muslims, Jews, and Christians living in the lands conquered by the Ottomans were thousands of refugees who found sanctuary in the empire: Jews fleeing from the Christian reconquest of Spain and Portugal and from oppression in Europe and Russia right up to and including the time of the Russian revolution and of Nazi persecutions in the twentieth century; Christians fleeing from revolutions in Central Europe and Russia; and, most significant in numbers, hundreds of thousands of Turks and other Muslims fleeing from massacre and persecution in Russia and the independent states established in South-eastern Europe during the nineteenth and early twentieth centuries.

Whether in the great cities and towns, where they congregated in their own districts, or in separate villages, the people of the Ottoman empire grouped themselves into autonomous communities usually called *millet*s, in which they preserved their own languages, traditions, religious practices, and customs, while coming together in a manner that might well have produced the sort of conflicts that have characterized Middle Eastern society since the Ottoman empire came to an end but did not. In considering Ottoman society, the question one must ask above all others is why there were very few conflicts; why were all of these peoples able to live together for so long in relative peace and harmony?

The multinational and multireligious society held together by the Ottomans for over five centuries has never been studied with the penetrating detail and understanding provided in this collection, the product of a conference, "The *Millet* System: History and Legacy," conducted at Princeton University in 1978 under the direction of Benjamin Braude and Bernard Lewis. In the introduction the editors stress that the alleged Ottoman persecution and oppression of minorities, claimed by so many in the West over the centuries, was largely a myth, but they add that relations among members of the different com-

munities were less the kind of perfect harmony and "tolerance" claimed by some Ottoman historians than a kind of mutual contempt, with the considerable persecution to which the Jews in particular were subjected by the far larger Christian communities being little hindered by an Ottoman ruling class, whose powers and authority to intervene were limited, even in times of its greatest wealth and prestige. Two general essays describe the position of the non-Muslim *zimmis* in Muslim society. The first, "The Concept of *Dhimma* in Early Islam," by C. Edmond Bosworth, describes the *Dhimma* in classical times. The second, "Transformation of *Zimmi* into *Askeri*," by I. Metin Kunt, describes its adaptation by the Ottomans, largely through the *Devshirme* system of recruiting young non-Muslims (and some Muslims) into the Ottoman ruling class.

The system in its broadest concepts is studied by Benjamin Braude in "Foundation Myths of the *Millet* System." He correctly points out that the internal structures of the different communities were not as similar as has been supposed and that the term *millet* was not in fact used for non-Muslim as well as Muslim communities until relatively late. One wonders, however, how enlightened the modern reader would be if authors applied to each group the different terms used locally during the periods under discussion.

Discussions of the individual *millets* are far more extensive for the Jewish community than for any of the others. Joseph Hacker, in "Ottoman Policy toward the Jews and Jewish Attitudes toward the Ottomans during the Fifteenth Century," emphasizes the hostility of the different groups. And Mark Epstein, in "The Leadership of the Ottoman Jews in the Fifteenth and Sixteenth Centuries," Amnon Cohen, in "On the Realities of the *Millet* System: Jerusalem in the Sixteenth Century," and Paul Dumont, in "Jewish Communities in Turkey during the Last Decades of the Nineteenth Century in the Light of the Archives of the Alliance Israélite Universelle," add more details. The Greek Orthodox *millet* is studied in some detail by Richard Clogg, in "The Greek *Millet* in the Ottoman Empire," who points out how the non-Muslim Slavic subjects of the sultan were systematically oppressed by the Greek religious hierarchy that controlled the system. Stavro Skendi also discusses Orthodoxy in "The *Millet* System and Its Contribution to the Blurring of Orthodox National Identity in Albania." The Armenian Gregorian community is far less systematically represented. Kervork Bardakjian, in "The Rise of the Armenian Patriarchate of Constantinople," describes the origins of the Armenian *millet* in the sixteenth century, while the late Hagop Barsoumian limits himself to its mercantile oligarchy

in the nineteenth century in "The Dual Role of the Armenian *Armira* Class within the Ottoman Government and the Armenian *Millet* (1750–1850)." A more general survey of the Armenian *millet* would have been useful.

Some of the most stimulating contributions come in the discussions of the *millets* in the last century of the Ottoman empire. Of these, the most enlightening are Kemal H. Karpat's "Millets and Nationality: The Roots of the Incongruity of Nation and State in the Post-Ottoman Era," which traces the effects of the rise of nationalism on the multinational Ottoman system, Charles Issawi's "The Transformation of the Economic Position of the *Millets* in the Nineteenth Century," Roderic H. Davison's "The *Millets* as Agents of Change in the Nineteenth-Century Ottoman Empire," Carter V. Findley's "The Acid Test of Ottomanism: The Acceptance of Non-Muslims in the Late Ottoman Bureaucracy," Feroz Ahmad's "Unionist Relations with the Greek, Armenian, and Jewish Communities of the Ottoman Empire, 1908–1914," Moshe Ma'oz's "Communal Conflict in Ottoman Syria during the Reform Era: The Role of Political and Economic Factors," and Dominique Chevallier's "Non-Muslim Communities in Arab Cities."

Although this collection breaks new ground in a variety of areas, it also demonstrates that many gaps in our knowledge remain. For one thing, the essays show very clearly that it is no longer possible to study any aspect of Ottoman society, including the different non-Muslim communities, without reference to Ottoman sources, many of which are indicated in Halil Inalcik's useful survey of "Ottoman Archival Materials on *Millets*." Other lacunae, however, also appear. Ottoman society was a whole, including Muslims as well as non-Muslims. There were Muslim *millets* as well as non-Muslim *millets*, and to study the one without the other creates imbalance and lack of perspective. Second, the volume ignores the explosive end to the Ottoman system, when the mutual toleration (or contempt) that preserved relative peace for such a long time dissolved and was replaced by national movements. These movements were built on the national identities that the Ottomans had allowed minorities to preserve. Unfortunately, these national movements became highly exclusive, intolerant, and fanatical political movements that expressed a desire for independence by violence, producing equal violence in return, and by a prejudiced and twisted view of the Ottomans throughout the West.

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JOEL BEININ and ZACHARY LOCKMAN. *Workers on the Nile: Nationalism, Communism, Islam, and the Egyptian Working Class, 1882–1954*. Princeton: Princeton University Press. 1987. Pp. xix, 488. \$75.00.

The writing of Arab labor history is not highly developed. One reason is that labor has had less influence on political life than have the military, organized religious movements, and secular parties of the Right and Left. But it is also because Middle East historians have preferred to write history from the top down, and, with some exceptions, they have been unsympathetic to the uses of class analysis in explaining change in the region. They argue that such analyses are not particularly useful for societies in which class has been subordinate to other forces.

A new generation of historians of the Middle East has begun to challenge this assumption. Informed by new methodologies being used in European and American history and equipped to use previously inaccessible local documents, these younger historians are committed to writing history from the bottom up. They are also more appreciative of class analysis. Although they acknowledge its limitations, they find its explanatory powers nonetheless compelling. This new generation's output has been uneven in quality, but this book is unquestionably on the high end of the scale.

Joel Beinin and Zachary Lockman convincingly demonstrate that the concomitant forces of British colonialism and Egyptian capitalism gave birth to a new class of organized urban workers, which made an impact on the struggle for independence as workers fought to become an independent political and economic force in their own right. An organized labor movement took shape earlier and much more rapidly in Egypt than it did elsewhere in the Arab world, a result of Egypt's earlier and more rapid integration into the European capitalist economy. But, because capitalist development was uneven, many industries remained small and organized along precapitalist lines. Class-conscious and activist workers were concentrated in the few large-scale industries tied directly to the cotton economy: transport workers before the mid-1930s and textile workers after the Second World War. The authors are careful to point out, however, that many organized workers in modern industries were discouraged from adopting an independent political outlook, the result of a combination of financial rewards and paternalism.

The authors skillfully weave the theme of paternalism through their study. By locating the struggles between the political elite and the workers for control of the trade unions in the wider context of the nationalist movement, they enrich our under-

standing of the familiar interplay of the British, the palace, and the Wafd and of the challenge posed by new, radicalized movements, in particular, the Communists and the Muslim Brothers. All of the major political parties sought to control labor, which suggests just how important it had become by the 1940s. Labor offered the possibility of organized support in the competitive arena of nationalist politics; at the same time, its potential for social instability required the scrutiny of bourgeois parties like the Wafd. At the core of some of the earliest struggles between the Communists and the Muslim Brothers were differing attitudes toward organized labor. While the Communists supported an independent workers' movement and promoted an active left wing within labor that contributed to major postwar strikes, the Muslim Brothers revealed strong signs of paternalism in their programmatic appeal and stewardship.

After contributing to the collapse of the Egyptian monarchy, the labor movement had to reckon with a powerful military regime that countenanced no opposition. The absence of widespread commitment within labor to an independent workers' movement, coupled with healthy doses of repression under Gamal Abdel Nasser, led to a bargain of historic proportion: in return for new labor legislation, including job security, trade unionists accepted direct government control of the workers' movement. In the end, workers failed to establish a viable, independent movement and faced a much more sophisticated brand of paternalism, one that offered certain real benefits. Beinín and Lockman reject the argument that union leaders simply betrayed the rank and file.

This study has many virtues. It is thoroughly researched and well written; its arguments are convincing. It brings into sharp focus the dynamics between class interests and nationalist politics, and it greatly enhances our appreciation of labor's contribution to the making of modern Egypt and of the emerging role of the Communists in the political life of the country. This book is, indeed, a model history of institutional politics in Egypt.

But even the best institutional histories have their shortcomings. Although the narrative is filled with dozens of trade unionists, there are few warm bodies. Virtually nothing is revealed about the daily life of ordinary workers, whether in the factories or at home. One is anxious to learn something about the ways new class consciousness and factory work altered family life. How did attitudes toward leisure and consumption, women and children, organized and popular religion, and quarter and village change? To be fair, the authors acknowledge that they were unable to address many of the probing questions now commonly associated with the new labor history. The

paucity of written sources was undoubtedly an obstacle. But what about oral histories and popular poetry, plays, and music? Such sources might have helped to elucidate these admittedly elusive strands of working-class life. Others will have to explore this new territory; they will be indebted to Beinín and Lockman for leading them to it,

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AFRICA

DAVID LEVERING LEWIS. *The Race to Fashoda: European Colonialism and African Resistance in the Scramble for Africa*. New York: Weidenfeld and Nicolson. 1987. Pp. xiii, 304. \$24.95.

The Fashoda incident of 1898 was a culmination of the European scramble for Africa. A French expedition under Jean-Baptiste Marchand crossed Africa in an epic journey to claim a position on the Upper Nile, while the British, from occupied Egypt, moved south, took the Sudan, and met the French at Fashoda, a ruined Egyptian outpost. Resolution of the ensuing crisis in Britain's favor shaped the colonial boundaries of East and Central Africa.

The story is familiar. This new book justifies a retelling by arguing that the African side has been obscured, important sources have not been tapped, and African resistance to European imperialism, of which the incident is a paradigm, has been underestimated. The book succeeds in surveying the complex political, diplomatic, and military interactions of European intruders and African polities. But David Levering Lewis's uncritical use of sources is ironically the book's greatest weakness: enthusiasm for the subject and a style of breezy assurance are unsupported by a command of the subject.

Thirty years after Richard Hill's *Egypt in the Sudan* (1959) and P. M. Holt's *The Mahdist State in the Sudan* (1958), it is disconcerting to observe a historian's uncritical reliance on war propaganda as source material. The Turkiyya is characterized as a regime of "ubiquitous corruption, extortion, and brutality" (p. 4), a conclusion devised by Europeans to justify colonial rule. The Khalifa Abdullahi is luridly described as a vicious sadist (p. 139) "surrounded by voluptuous concubines" (p. 144), in keeping with the propaganda of his British enemies. More seriously, sources are inaccurately quoted and cited, misread and misinterpreted. A few examples must suffice. The author charges the khalifa with burning religious texts (p. 138); on examination, his source ascribes this act to the Mahdi. Lewis contends that in 1894 troops "rampaged the city [Omdurman], pillaging

houses, accosting merchants, forcing traders to pay protection, and occasionally killing" (p. 145). His source, a Sudan *Intelligence Report*, in fact refers to "robbery and plunder" but says nothing about assaults on merchants, protection money, or murders. Again he misquotes the khalifa as telling the Jaaliyyin, "If you die, you go to hell, and if you live, you work like donkeys" (p. 145); his source, another *Intelligence Report*, reads, "... you work on donkeys" (my emphasis), not the same thing. Frequently no source is given for "factual" material, for instance, the charge that the khalifa escaped "in disguise" from Omdurman (p. 205). Such liberties with already suspect sources render the book unreliable.

Minor errors of fact abound; only a few may be mentioned. "Mahdi" does not mean "messenger of Allah" (p. 7). Isma'il Pasha did not abolish slavery (p. 28). El Obeid is not in Darfur (p. 28). Slatin did not surrender at El Fasher (p. 42). "Zande" is not a place (p. 61). Omdurman was not named by the Mahdi, and the word does not mean "place of pearls" (p. 139). The Ashraf did not claim "descent from the Mahdi" (p. 140). Jabal Qadir is not in the southern Sudan (p. 228). The astonishing assertion that Junker and Schweinfurth were "the first Europeans to enter the Lower [sic] Sudan" (p. 4) betrays ignorance of a whole generation of travelers (and of their accounts). Diacritical marks are scattered haphazardly, and some names, even those cited in the acknowledgements, are wrong.

Cumbersome and uneconomical references—no superscriptions in the text; only page numbers and key phrases at the end of the book—make it difficult but not impossible to detect numerous mistakes: incorrect titles, pages noted that do not contain the information ascribed to them, one book cited when another is meant, and so forth. An impression of rare sources is misleading; that the National Records Office in Khartoum "made available the invaluable 'Critical Edition of the Memoirs of Yusuf Mikha'il'" (p. xiii) rather inflates its provenance as a University of London thesis. Consistent with a novelistic treatment is the author's unbridled prose. Thus, a governor "permitted extortion and corruption to blanket his domain like sands from a desert storm" (p. 28). Two or three adjectives are employed where none would do, for example, "the handsome, vigorous, and plain-spoken Urabi" (p. 28).

The result is a highly readable adventure-narrative, inaccurate and unsupported by wide reading and a critical use of sources. Sections dealing with European, especially French, politics inspire more confidence than those on Africa. Repetition of old calumnies is especially unfortunate in light of Lewis's professed purpose. The sacrifice of

pedestrian accuracy for narrative exuberance places the book in the category of those late nineteenth-century works whose authors, Lewis says, "almost always saw only what they wanted to," whose "fascination with Africa evolved into distorted familiarity" (p. 12).

M. W. DALY

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CHARLES H. AMBLER. *Kenyan Communities in the Age of Imperialism: The Central Region in the Late Nineteenth Century*. (Yale Historical Publications, number 136.) New Haven: Yale University Press. 1988. Pp. x, 181. \$27.00.

Charles H. Ambler has written a finely crafted and powerful book that presents a new and compelling approach to the reconstruction of precolonial African social history. Although his description of this approach as "people's history" (p. 7) is a bit hyperbolic, the emphasis on small-scale communities and societies in a regional framework provides a view of social history that is refreshing and provocative.

Ambler introduces us to his region, community by community. I say his region not to demean his selection of boundaries or terrain but to indicate that the region he describes cannot be found on any previously drawn provincial, district, or ethnic map of central or eastern Kenya. It centers on the upper Tana River, which is the backbone of Ambler's area, tying together the northern parts of the Kamba districts of Kitui and Machachos with Embu, Mbeere, and the southern parts of Meru and Kikuyuland. This region cuts across colonial and postcolonial districts that use the Tana as a "natural" boundary to divide the area into exclusive "tribal" districts, imposing these "cultural-linguistic traditions" (p. 9) and tribal boundaries on a far more fluid nineteenth-century reality. For Ambler, this misperception of the past is the central problem in the historical analysis of precolonial African society.

In Ambler's vision of the nineteenth century, the tribes of Kenya vanish. They are replaced as units of analysis by "the experience of individuals and families and the institutions that dominated people's lives: neighborhood, the community, the region" (p. 7). The placement of the apostrophe is crucial as people's history is substituted for the conventional history of peoples. "No settled and exclusive ethnic order defined social and economic relations in nineteenth-century central Kenya" (p. 7). The use of community and region as a lens for historical observation proves in Ambler's capable hands an incisive tool. In less capable hands, there is a danger of discarding ethnic or

tribal analysis for an even more distorted vision resulting from fragmentation and infinite reduplication of the units of analysis. But for now this study should serve to further undermine the already weakened state of the tribal concept in African studies.

The author avoids the pitfalls of fragmentation by placing his subethnic communities in the broader context of interethnic regions. Indeed, ethnic units appear to have no impact on the patterns of local production and regional exchange described by Ambler. Instead, we find that the lives of people are not shaped or ordered by their tribal affiliation but by the patterns of local trade and specialization in production that flow from changing ecological conditions and local patterns of comparative advantage. If I may oversimplify Ambler's subtle analysis of the "complex mass of connections" (p. 5), the region can be divided into frontier versus settled zones. Migwani and Mumoni in northern Kitui District represent the frontier tradition of high mobility, low population density, and pastoral and commercial specialization, resulting in larger status and wealth differences and situational authority (big men). Embu in the Mount Kenya highlands represents the settled and stable zone of higher density and agricultural specialization. Here agricultural self-sufficiency breeds cultural isolation, social equality, and the established authority of elders. These two zones were linked by a complex and delicate system of exchanges of food, cattle, and other basic goods conditioned by environmental and "economic complementarity" (p. 50).

The disruption of this harmonious but fragile balance has been attributed to powerful external influences (the slave and ivory trade and the advent of colonialism) by both scholars and local tradition. Ambler prefers to keep his regional focus by locating the impact of the growth and decline of long distance trade on the complementarity and ecological balance of his central Kenya communities. The breakdown of local self-sufficiency and regional autonomy is what induces the dual crisis of ecological disaster and colonial invasion. The clear and compelling narration of this transformation gives the study its poignant climax: the Great Famine (1897–1901) is not only a watershed in the history of central Kenya but also the dramatic symbol of the "time of troubles" endured by virtually all African communities in the age of imperialism.

Ambler's writing is clear and concise; the notes at the foot of the page are useful and unobtrusive. The 172 interviews conducted over fifteen months of field research are an important indication of the strenuous requirements of regional history. I might quibble that more information on the selec-

tion of informants and interpreters would have added to my interest. A full list of published references rather than a select and incomplete one would have simplified scholarly scrutiny. Considering that the author and publisher pack an impressive amount of new research and probing analysis into a mere 181 pages of text, notes, sources, and index, it would be mere carping. Instead, we should thank Ambler and Yale University Press for bringing this important work to the historical community and its neighbors in the geographical and anthropological region.

EDWARD I. STEINHART
Texas Tech University

SANDRA T. BARNES. *Patrons and Power: Creating a Political Community in Metropolitan Lagos*. (International African Library.) Bloomington: Indiana University Press, in association with the International African Institute. 1986. Pp. x, 261. \$32.50.

Mushin is one of the largest suburbs of metropolitan Lagos, and throughout Nigeria it has the reputation of being an especially disorderly, even dangerous, part of the capital city. Whenever explanations are sought for spectacular outbreaks of armed robbery, drug smuggling, and political violence in the capital, the population of Mushin usually figures prominently. Sandra T. Barnes acknowledges that it was the "frontier-like quality" of Mushin (p. ix) that captured her attention in the early 1970s. In this book she provides a detailed anthropological analysis of the suburb's contemporary political organization.

By way of historical background, Barnes argues that, throughout the late nineteenth and early twentieth centuries, the many villages scattered across this area were not of sufficient consequence to merit formal government. The indigenous Awori had to rely on the patronage of influential chiefs resident in Lagos in order to put their interests before their colonial overlords. Subsequently, as the area became more populated and the economy more diverse, the role of political mediator between the population at large and the colonial government was taken over by a rising elite of settlers. By the middle of the present century, the ownership of urban land regularly underpinned political leadership.

Barnes's account of clientage in Mushin's past is sound, but it is in her analysis of contemporary processes that one finds the meat of her study. After detailing how the property owners of the suburb used their common economic resources to build extended family circles, ramified neighborhood networks, and their public reputations, Barnes charts those political careers that have

successfully penetrated the political and bureaucratic power structures of Lagos. The leaders from the suburb are by no means united, as is evidenced by their intense factional struggles for control over a major Mushin market. But for the more successful the rewards are substantial, notably in their legal recognition as urban chiefs. These offices became all the more valued with the imposition of military rule in 1966. Prominent leaders, in return for their compliance, acquired both the symbols of traditional authority and new opportunities for modern patronage.

Barnes's major concern is thus with the pragmatic politics of a rising propertied elite in a metropolitan environment in constant flux. Because these patrons are especially concerned about their political legitimacy, Barnes could have said more about how the ordinary residents of Mushin adjudicate the devious strategies of their well-heeled leaders. More important, since the population of Mushin is 1.5 million, one wonders whether the patron-client relations so ably detailed by Barnes exhaust the major political processes operating in the suburb. It is somewhat paradoxical that, while Barnes was initially attracted to Mushin's frontier-like disorder, the image of the suburb that emerges from her study is one of a relatively ordered and integrated arena. One cannot help but speculate that the political community on which Barnes focuses has numerous rivals, ranging from disputatious labor unions to itinerant street gangs, who are contesting control over the varied segments of this vast urban population. These caveats apart, *Patrons and Power* is an important contribution to our understanding of political relations and processes in present-day sub-Saharan urban Africa.

ADRIAN PEACE
University of Adelaide

IVOR WILKS *et al.*, editors. *Chronicles from Gonja: A Tradition of West African Muslim Historiography*. Arabic texts edited and translated by NEHEMIA LEVIZION. (Fontes Historiae Africae, Series Arabica, number 9.) New York: Cambridge University Press. 1986. Pp. xii, 258. \$59.50.

This volume is one more addition to the wealth of publications on the Ghana past, not on Asante, for which the principal editor, Ivor Wilks, is noted, but on the Gonja kingdom and erstwhile tributary of Asante to the north. The work is a rich resource of a rare collection of primary documents, in Arabic, of local authorship that chronicle the early history of Gonja from its creation in the mid-sixteenth century by Malian conquerors through its consolidation to its later conquest and incorpo-

ration into the Asante empire in the mid-eighteenth century.

The body of the publication consists of five main documents that are presented and closely examined under separate chapter headings as follows: the *Amr Ajdādīnā*, which records the history of the foundation of Gonja; the *Kitāb Ghanjā*, chronicling the course of Asante expansion into Gonja; the *Ta'rikh Ghunjā*, a history of Gonja of sorts; the *al-Kalām Māghu Sansani*, which narrates the story of the foundation of Sansanne Mango, later the capital of the new Chokosi kingdom east of Mampurugu; and the *Kalām Malūk wa Mamālikihim*, a brief document describing the autochthonous groups among the Ngbanya ruling class. The treatment of these documents is preceded by a substantive contextual discussion in which the editors, first drawing on Wilks's earlier publications, argue that the Gonja state system was the product of the transition from a bullion-based economic "world-system" to the Atlantic, labor-based "world-system" and, second, trace the history of the broader historiographical tradition in pre-literate Gonja. The work concludes with an examination of documents of a slightly different variety: selected pieces of correspondence between Gonja and Asante, prepared by the Gonja *ulamā* (Muslim clerics) in the nineteenth century.

The Gonja chronicles must be seen in the context of Muslim historiography, which they so much exemplify in structure, both in the *khbar* form, with its scanty narration of events, and in the more detailed annalistic form where events are interconnected and more explanatory material is infused. Although recognizing the religious overtones in the product of a class of Muslim savants who undoubtedly enjoyed the patronage of secular (non-Muslim) rulers, Wilks and his colleagues argue nonetheless that, even where documents underwent later redaction and revision, there appeared no ideological motivation to manipulate the story of Gonja's creation and consolidation because Gonja was a conquest state right from the start (unlike Asante where tradition appears to have been manipulated to establish a consensual basis of society).

The significance of the volume must be seen in the wealth of information it contains, particularly on political history. The documents, especially the *Kitāb Ghanjā*, provide historical information that is important for a reconstruction of events not only in Gonja itself but also in some of the adjoining areas including Dagomba, Mampurugu, and Wa (in northern Ghana), Bobo Dioulasso (in southern Burkina Faso), and Bonduku, Buna, and Kong (in the eastern Ivory Coast). The texts relating to the history of the divisions that were eligible for paramount chiefships have a relevance for local ad-

ministration, particularly in claims to office, status, and land, a factor that the British colonialists did not fail to recognize in the early twentieth century.

Emanating from a project in the collection of Arabic manuscripts initiated by the Institute of African Studies at the University of Ghana in the early 1960s and benefiting from decades of investigation on the part of the editors, this volume places at the disposal of the historian an invaluable resource that should corroborate other historiographical sources, such as the larger corpus of orally transmitted material and drum history, and that should complement secondary works, notably the publications of Jack Goody. The relevance of this collaborative publication, however, extends as well to students interested in issues associated with non-Western literacy, oral tradition, and, above all, Islamization in Africa.

JOSEPH K. ADJAYE
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DAVID D. LAITIN and SAID S. SAMATAR. *Somalia: Nation in Search of a State*. (Profiles/Nations of Contemporary Africa.) Boulder, Colo.: Westview or Gower, London. 1987. Pp. xvii, 198. \$28.00.

This collaborative effort by David D. Laitin, an American political scientist, and Said S. Samatar, a Somali historian, is a highly readable and stimulating introduction to a country where drought, boundary wars, and massive refugee problems have frequently made headlines in the past decade. The series in which this book is published, under the able general editorship of Larry Bowman, has an orientation toward the present; more than half of the book deals with events since Somalia's independence in 1960. Nonetheless, chapters 1 and 3 offer competent summaries of the precolonial and colonial history of the Somali peninsula, and chapter 2 vividly delineates the major elements of traditional Somali society—a harsh and arid environment, a camel-keeping culture, an extended kinship system, and an egalitarian, democratic political culture in which the oratorical skills of poets and clan elders can alternately instigate or mediate group conflict—which continue to inform the country's public life. Chapters 4 through 6 deal with the government, economy, and foreign policy of postindependence Somalia; they are lucid and free of jargon and successfully distinguish policy rhetoric from measurable performance. The final chapter takes a speculative look at what the future might hold for a poor country whose strategic location at the southern end of the Red Sea has made it both a player and a pawn in the big-power politics of the region. Throughout, the authors blend sharp

analysis and illuminating anecdote in a manner that both Africanists and nonspecialists will appreciate.

One of the central themes of the book is that the citizens of Somalia share a common language, Islamic faith, pastoral tradition, and rich poetic heritage—giving them a sense of national identity that few contemporary African states can claim—but their political culture based on kin and clan allegiances lends itself even in modern contexts to internal factionalism and recurring political anarchy. At one point in the 1960s, "political parties proliferated to the point where Somalia had more parties per capita than any other democratic country except Israel" (p. 69). The resulting electoral chaos proved too much for a corrupt and paralyzed civilian leadership to handle. The military took over in a coup in 1969 and has ruled Somalia ever since. Initially, the military regime of President Mohamed Siad Barre sought to overcome the parochialism of clan and to construct a society along socialist lines. But a series of setbacks (economic dislocation as a result of drought and refugee influx, military defeat at the hands of neighboring Ethiopia, and conservative Muslim reaction to Soviet-inspired policies that promoted women's rights and national secular education) cost the regime its early popularity; the leadership reverted once more to the manipulation of clan alliances, playing old rivals against one another and placing trusted kinsmen in key government and military posts. The supratribal unity and emotional nationalism that had been so evident in the Somalis' ill-starred efforts to conquer the Ogaden region of neighboring Ethiopia in 1977 and unite with their fellow Somalis foundered at the domestic level in the intense rivalry among clans for political power and influence.

Throughout the book, there is greater emphasis on the structure and performance of the modern state than on those social and economic processes that have been at work on the local and regional levels or on those that transcend the formal boundaries imposed on the country at the time of the European colonial occupation. I am thinking here of the extensive clandestine commerce in livestock and qat (a mild narcotic plant) with Somalia's neighbors (Kenya, Ethiopia, Djibouti); the tremendous impact on the domestic economy of earnings remitted by the two hundred fifty thousand Somalis who live and work outside the country in the Middle East, eastern Africa, and Europe; the currents of Islamic discontent that lie just below the surface of contemporary public life; and the rapid urbanization that is transforming the demographic landscape and changing the consumer patterns, values, and political consciousness of Somalia's younger generation. Perhaps it is still

too early to assess the effects of these phenomena on clan loyalties, which heretofore have seemed impervious to the forces of capitalism, individualism, or Islamic communalism; but to focus exclusively on the nation-state, its policies, accomplishments, and shortcomings, may obscure the most significant potential sources of change in Somalia's political culture over the next decade.

In sum, given the emphasis on states in this series and its constraints on length, Laitin and Samatar very effectively demonstrate how the historical experience of a people conditions its contemporary cultural attitudes and, in conjunction with the pressures imposed by the world economy and the strategic maneuverings of the superpowers, continues to shape its domestic and foreign relations.

LEE V. CASSANELLI
University of Pennsylvania

PAUL MAYLAM. *A History of the African People of South Africa: From the Early Iron Age to the 1970s*. New York: St. Martin's. 1986. Pp. ix, 259. \$32.50.

During the last twenty years, there has been a vast increase in the publication of specialized books and articles on the history of South Africa by scholars in Britain and the United States as well as in South Africa itself. Paul Maylam has synthesized much of this work, and his account of the peoples of South Africa is a bold and impressive achievement. Maylam picks his way carefully (if sometimes somewhat tediously) through the ideological minefields that clutter the landscape and comes out with judicious, pluralistic conclusions.

He starts with a sound attempt to make sense of the controversies concerning interpretations of new archaeological data about the precolonial history of the region. After that, his main themes are African loss of political independence, which was followed, more gradually, by the Africans' loss of economic independence and the causes of both, namely, the inadequacy and impoverishment of their reservations (later "Homelands") and their exploitation as agricultural and industrial laborers for whites. Using a class analysis as his major explanatory tool, Maylam also summarizes the successive phases in African resistance to white domination in the twentieth century, from cautious, peaceful reformism to revolutionary demands and actions.

Maylam's work has two original features. Focusing on Africans, he says scarcely anything about the "Coloured" and Asian people of South Africa, while the whites who dominate the region are only present in the background. This is all very well for readers who know the main outlines of South

Africa's history, but others will be left in ignorance of the processes that have produced the political, economic, and military power of the South African state that maintains African subjection. Maylam also divides the history of the region into three periods, with cutoffs at 1830 (marking the end of the African wars sparked by the rise of the Zulu kingdom under Shaka) and 1900 (marking the end of African political independence), and he subdivides the final period at 1936. His division has the effect, among other things, of seriously underestimating the significance of the year 1948, which brought into power the Afrikaner National party that has ruled the country ever since with its policy of apartheid. In its impact on Africans, the party and the decades of its rule have been very much more than a mere continuum of the past.

LEONARD THOMPSON
Yale University

ROBERT VICAT TURRELL. *Capital and Labour on the Kimberley Diamond Fields, 1871-1890*. (African Studies Series, number 54.) New York: Cambridge University Press. 1987. Pp. xviii, 297. \$42.50.

Within the past six years a spate of articles and books has substantially revised our understanding of the formative years of South Africa's diamond-mining industry. Robert Turrell, author or joint author of several of these articles, repeats and refines his revisionist contributions in this monograph.

Denied access to the De Beers Consolidated Archive in Kimberley, Turrell used notes made by other researchers who exploited that archive. His very extensive evidence also comes from the Standard Bank Archive in Johannesburg, which is a treasure trove for the economic historian, government records in Cape Town and London, and the papers of Frederic Stow, a well-informed diamond magnate.

As his preface makes clear, Turrell is inspired by the radical historiography generated by the workshops on southern African history lodged at the University of London. Shula Marks, a key member of these workshops, supervised the thesis from which the monograph emerged.

The most interesting sections of the book are its descriptive passages. These cover the mechanization of the mines, marketing of the precious stones, financing firms, working conditions, and the subterfuge used by illicit diamond buyers and the state agents who tried to catch them.

The book takes issue in particular with four historiographical problems. Turrell argues that previous analyses have ignored the receptivity of

the colonial state to local as opposed to foreign capital during the Black Flag Revolt of 1875. Those analyses also ignored the important role black and white share workers, diggers who did not own claims, played in the productive process.

The introduction in 1885 of the closed-compound system for black workers was, he agrees, the result of mineowners wanting to prevent theft. But it was also in no small measure dictated by the need to control and manipulate labor to make the most of mechanized underground production.

Turrell also takes up what he calls the "pure mythology" (p. 206) of the amalgamation struggle of 1889–91 to monopolize Kimberley's diamond production. He shows how Cecil Rhodes was not locked in a struggle for control with Barney Barnato. Rather, Barnato had been bribed with flattery, opportunities for insider share trading, and promises of diamond sale profits. Turrell also suggests that Rhodes's fixation on Central Africa was a logical outgrowth of his diamond-mining interests and not necessarily part of a grandiose scheme to enlarge the British empire. The region was a source of labor, and it could also yield diamonds that would undermine the price that Kimberley stones fetched.

Finally, as part of an analysis using the work of labor historians on the sources, recruitment, initiatives, and expectations of black miners, Turrell examines mortality rates. This examination considers death rates in mining in the Transvaal and southern Rhodesia and leads to a discussion of working conditions well into the twentieth century, beyond the time frame in which most of the book is cast.

This particular problem with chronology is symptomatic of other flaws, stylistic as well as organizational, that detract from the effectiveness of the presentation. Although the monograph allowed Turrell to correct a printing error in a table that appeared in a previous article (p. 257, n. 2), he did not take the opportunity to integrate fully into his text information from numerous painstakingly constructed tables. Photographs are, however, very well used. There is a misprint on page 39, Cradock is misspelled on page 57, and an antecedent is misplaced on page 174.

The author is light-handed in condemning the machinations of Kimberley capitalists and imposing what he calls a materialist understanding of African history. So the reader can derive a good deal of insight from this informative book, neither swayed nor put off by an overbearing argument.

ROBERT KUBICEK

University of British Columbia

WILLIAM BEINART and COLIN BUNDY. *Hidden Struggles in Rural South Africa: Politics and Popular Movements in the Transkei and Eastern Cape, 1890–1930*. (Perspectives on Southern Africa, number 40.) Berkeley and Los Angeles: University of California Press or James Currey, London, or Ravan Press, Johannesburg. 1987. Pp. xxvi, 326.

If indeed, as Mies van der Rohe said, "God dwells in the details," the many-faceted visage of Clio surely gazes at us with subtle and variegated expressions through these densely written pages. William Beinart and Colin Bundy, respected exponents of South Africa's revisionist historiography, present eight detailed studies of social and cultural change, quite literally at the grass roots of the Eastern Cape and Transkei in the late nineteenth and early twentieth centuries. The special value of their effort, clarified in a lengthy analytical introduction, is to explicate the richness and variety of circumstance and experience in communities and individuals at formative but still inchoate stages, without submerging them in a priori categories or judgments.

We follow headmen and commoners, traditionalists and modernizers through episodes of subordination and alienation, of mediation and confrontation, of efforts at organization, representation, and empowerment that compose an extraordinarily complex picture of creative and autonomous responses to the advancing colonial state and economy. The anatomy of an insurrectionary scare reveals ambiguities cutting across official, settler, and African communities—and the divisions within them. The story of a mission-educated headman demonstrates the value of biography as a window to social structure and change. "Collaborators" emerge kaleidoscopically as protagonists of change and resistance, mobilizers and articulators of popular consciousness. Labor migrancy, in reality many different experiences, raises questions of methodology for understanding its true effects on worker consciousness. An episode of resistance to stock dipping gives insight into popular alignments against both the traditional authorities and the educated elite. A women's movement in Herschel District reveals the changing nature of rural protest as urban-based phenomena such as the African National Congress and the Industrial and Commercial Workers Union (ICU) and millennial and Africanist movements such as Wellingtonism and Garveyism spread through rural communities after World War I. The rural experience suggests that popular consciousness and nationalist thinking were far apart and that "the balance of social forces" remains "ill understood" (p. 261). Rural consciousness, as rooted in ethnicity rather than in

class, must therefore be distinctly recognized and incorporated into material analysis. Lastly, a study of the shifting ideology of the Independent ICU of East London about 1930, focusing on its failure to accommodate popular radicalism, elucidates the "conundrums" (p. 316) of leadership that be-deviled both the great ICU and its local successor, contributing to their confusion, frustration, and failure.

The authors clearly aim at issues of definition, seeking to enrich the new historiography with rural themes. Their conclusions remain carefully and responsibly restrained, but they point successfully to the fuller and wider social contexts of resistance on which the day-to-day realities of Africans' lives impose the necessity to define anew, with a greater sense of variability, the character and processes of social change and class formation. They search for "a generic form of rural politics" (p. 35), challenging conventional assumptions of both liberals and radicals who focus on the urban industrial arena. Pointing out that rural politics had its distinctive material base in the productive resources of land and stock, of family and communal economy and culture, they argue that rural resistance was not simply a reactionary impulse but also a creative search for new means and modes to protect clearly perceived interests.

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ASIA AND THE EAST

CHRISTOPHER I. BECKWITH. *The Tibetan Empire in Central Asia: A History of the Struggle for Great Power among Tibetans, Turks, Arabs, and Chinese during the Early Middle Ages*. Princeton: Princeton University Press. 1987. Pp. xxii, 269. \$38.00.

The Tibetan empire that flourished between the seventh and the ninth centuries is little known to historians. We are told vaguely of its existence in the annals of its neighbors, but the details are lacking. Thus, its significance escapes our attention. Christopher I. Beckwith's book is an attempt to reconstruct the turbulent history of the struggle for dominance in Central Asia by the Tibetan, Chinese, Turkic, and Arabic powers during this period and specifically the rapid rise and gradual decline of the Tibetan empire. Using materials drawn from medieval Chinese, Tibetan, Turkic, and Arabic sources, the author has succeeded in this pioneering work in rescuing from obscurity an important chapter in Asian and world history.

Unfortunately, limited by the nature of the sources and the still-undeveloped field of medieval Tibetan studies, Beckwith's account becomes an unrelenting compilation of battles won and lost

and cities captured and abandoned. The two maps provided in the text are of little help in assisting the reader to gain a clearer picture of the events described. There is little information on the social and political structure of Tibetan society, which sent its armies east to the capital of T'ang China, west across the Pamirs into outposts of the expanding Muslim world, north to the oases of the Tarim basin, and south to the semitropical Nan-Chao kingdom. Aside from noting the excellent quality of Tibetan armor, Beckwith does not tell us how the Tibetan armies were organized, how they fought and administered their conquered territories. We have now a clearer idea of the territorial extent of the Tibetan empire, but its leaders are still little more than mere names to us. And we remain in doubt of the source of Tibetan energy that inspired the Tibetans to challenge their powerful neighbors.

In an interesting epilogue, the author argues forcefully for recognition of the advanced culture of the Frankish empire in Western Europe and the Tibetan empire in Asia. But the evidence adduced for the Tibetans is so scanty and speculative that it would probably take a great deal more work from scholars laboring in the field to make the case convincing. On the other hand, his discussion of the proper classification of the Tibetan language and developments in the field of Tibetan studies is both helpful and welcome.

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THOMAS T. ALLSEN. *Mongol Imperialism: The Policies of the Grand Qan Möngke in China, Russia, and the Islamic Lands, 1251-1259*. Berkeley and Los Angeles: University of California Press. 1987. Pp. xvii, 278. \$35.00.

The great Mongol empire of the thirteenth century was principally created under the reigns of Chingis (d. 1227) and Ögödei (d. 1241). By Khubilai's time (r. 1260-98), however, it was no longer a single entity but rather a collection of four qanates (older form: khan) possessing distinct and sometimes mutually antagonistic interests. So much is generally agreed upon. But what about the intervening period and especially the little-studied reign of Möngke, 1251-59? (To be sure, conquests continued in the 1250s, most notably of Persia.) Does Möngke's reign fall into the early, unified conquest phase or into the subsequent centripetal phase? This question is the subject of Thomas A. Allsen's absorbing study, and his answer is unequivocal: the Mongol empire still functioned in unified fashion under Möngke whose

dictates were taken quite seriously by his junior qans. Allsen's attempt to demonstrate this thesis proceeds, inevitably, along comparative lines. After treating Möngke's early career, succession to the throne (particularly bloodstained), his relations to fellow Mongol rulers and other heads of state, and the political-administrative means he adopted, Allsen takes up in successive chapters the empire-wide registration of population, imposition of taxes, and recruitment of a work force. All of these constituted Möngke's "program of resource mobilization" to which he joined a program of expansion, thereby keeping both centralizers and expansionists at court and elsewhere happy.

The difficulties standing in the way of an exercise such as Allsen's are formidable: sources are in many areas scant or nonexistent; they are in at least a dozen languages (since this was a "global" empire); and the Mongols themselves were poor writers of history (or anything else). The author brings some important tools to his task, specifically a working knowledge of Chinese, Persian, and Russian. Russian is particularly valuable in enabling him to tap the rich vein of Russian scholarship on the Golden Horde and Central Asia. Methodologically, the procedure used is to confirm in local or regional sources anywhere compliance with orders from the center, an approach already widely employed by the Russian historian L. N. Gumilev. Often these data are few and far between, yet Allsen makes a persuasive (if not compelling) case that the Mongol empire in the 1250s still enjoyed considerable cohesion and a strong sense of a center. His discussion is precise, lucid, and rich, especially on the level of imperial policies and operations. Would a closer look at local conditions modify this picture? Possibly. And would it reveal equally difficult challenges to centralization within the qanates themselves? Probably. Although the problem of limited source materials is an insoluble one, sources on China can take us far. Here Allsen has not fully exploited the Chinese secondary literature, and, as he acknowledges, he has made no use of the enormous body of Japanese scholarship. Yet this book is without any doubt an important contribution, a first-rate work of synthesis really, which anyone with even the slightest interest in thirteenth-century Eurasian history should read.

CHARLES A. PETERSON
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TANIGAWA MICHIO. *Medieval Chinese Society and the Local "Community"*. Foreword and translated by JOSHUA A. FOGEL. Berkeley and Los Angeles: University of California Press. 1985. Pp. xxxix, 141. \$24.95.

Several exits have been sought from the impasse in which Western studies of "medieval" (roughly the first millennium A.D.) China have languished. The scholars of literature and religion who have dominated the field have sought an ever more sophisticated grasp of the surviving relevant texts, which are problematic on many levels. Observers cognizant of twentieth-century breakthroughs in the study of earlier periods of Chinese history have called for a stronger commitment to nontextual paths of analysis, such as archaeological, climatological, and so forth. Historians of the period have tended to rely on the impressive but risky crutch of Japanese scholarship. That great body of work has been plundered for information and points of entry into the Chinese texts, often with no attention paid to the scholarly tradition of which it is part.

The translator of the work at hand, Joshua A. Fogel, has done more than anyone else to focus Western attention on the internal dynamics of Japanese Sinology. In a series of path-breaking studies, he has traced the careers and intellectual commitments of a number of major figures. Here he has chosen to present Tanigawa Michio, the leading medieval historian of the Kyoto school, in his own words in the form of a long theoretical essay on the application of the concepts "feudal" and "medieval" to China and how we might transcend them.

Part 1 of the translation, "Chinese Society and Feudalism: An Investigation of the Past Literature," is primarily a treatment of Japanese work, emphasizing the contributions of Ishimoda Shō, Hori Toshikazu, Nishijima Sadao, and Niida Noboru, although Max Weber, Edwin O. Reischauer, Etienne Balazs, Karl A. Wittfogel, and Jean Chesneaux figure in the discussion. Part 1 reflects critically on the application of Eurocentric models, whether derived from Marxism or modernization theory, to non-European societies. Tanigawa concludes that, as feudalism is merely "the European form by which the ancient world was sublated" (p. 64), the task before us is to trace the distinctively Asian process of realizing such a taking away, gaining thereby both an understanding of medieval China and an appreciation for the indigenous dynamism of Asian history.

Part 2 of the translation, "The Medieval Period in China: Society in the Six Dynasties and the Sui-T'ang Periods and the 'Community,'" presents the controversial solution to the debate offered by Tanigawa and the late Kawakatsu Yoshio. Community (*kyōdōtai*) theory proposes a web of moral and ethical communal ties, transcending social class, as the fundamental mechanism of Chinese historical development. As community is nowhere clearly defined, and its aspect of naïve idealization

never adequately dealt with, it is hard to imagine community transforming Western approaches to medieval China. Fogel is too much the gentleman to dwell on the psychodynamics whereby a Japanese academic projects such a vision into the Chinese past. He has put us deeply in his debt by making readily accessible a major contribution to contemporary Japanese debate on medieval China.

Specialists will want to consult John Lee's notes on the translation in the *Journal of Asian Studies* (February 1987).

DENNIS GRAFFLIN
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RALPH R. COVELL. *Confucius, the Buddha, and Christ: A History of the Gospel in China*. (American Society of Missiology Series, number 11.) Maryknoll, N.Y.: Orbis. 1986. Pp. xv, 285. \$14.95.

This unique book is an analytical record of the disposition of Christian ideas in Chinese prose. Ralph R. Covell, a former missionary, a China specialist, and a theologian, chronicles the work of Nestorians, Jesuits, and Protestants in the missionary effort of fourteen hundred years to bring the Christian message to the Chinese. The enterprise involved confrontation with the rules of life associated with the teachings of Confucius and the notions of faith and the natural order in Chinese Buddhism. Covell concludes that the missionaries were able to create a gospel that addressed and accommodated Chinese ideas, but they were unable to inspire an indigenous gospel. The gospel in Chinese has not amounted to a Chinese gospel, for the Christian message in China has not come free of its foreign connection and thus has not reached the broad masses of the Chinese people.

The most common form of the gospel in Chinese has been tracts outlining "foundational truths" by focusing on God, Christ, human nature, and other doctrinal subjects (p. 94). An excellent example of this category is W. A. P. Martin's *Tiandao suyuan* (Evidences of Christianity), published first in 1854 and in print through more than thirty editions over the next six decades. Covell's splendid account of Martin's classic permits a rare view of what Covell presents as the contextualization of the gospel. In this regard, a Chinese concept such as *li* (principle, reason) coexists with the supremacy of the Christian God, who, in Martin's summation, implants *li* in all things.

Covell's insistence on the failure of indigenization is somewhat at variance with his own representations of the Taiping rebels' gospel-inspired ideology, the function of Christianity in reformers' efforts to "save" China in the early twentieth

century, and the impact of the work of over six hundred Chinese churches founded in China between 1901 and 1931. The author's argument that the "gospel of power," or Western imperialism, precluded Chinese acceptance of the Christian message is less convincing in this light (p. 253).

Covell's placement in the middle of the book of two chapters on Buddhism creates a certain disjointedness of historical flow. Also disturbing to me are some errors in the romanization of Chinese names and terms, some problems of source specification, and some confusion in the use of both parenthetical and endnote references. Such technical weaknesses aside, the gospel in Chinese remains uniquely and thoughtfully available by means of this book, which provides information and insight to students of missions and China alike.

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JOHN KING FAIRBANK. *China Watch*. Cambridge: Harvard University Press. 1987. Pp. viii, 219. \$20.00.

This volume reprints some of the book reviews John K. Fairbank has written since the 1970s. Although collections of book reviews are extremely rare, both the author and the publisher are justified in having undertaken the project because of the central position Fairbank has occupied in American studies of China. Many of the authors he reviews are his former students, and others are at least heirs to the scholarly legacy he has established and fostered. The book therefore attests to his continued and loving interest in his junior colleagues' work. This may be the secret of his longstanding influence. In addition to his own research, he has kept himself abreast of publications related to China, so that there is always dialogue between his work and the work of others.

Although the books he reviews vary in content and approach, Fairbank examines them in terms of the conceptual framework he has maintained over the years: the contrast between Chinese and Western (in particular, American) cultural values and traditions. Having devoted his lifetime to the study of China, he finds that "the history of China in American minds shows a high degree of continuity on the level of cultural values" (p. 4). Many American writers, he says, "have not got far beyond the values and underlying attitudes" of S. Wells Williams, who wrote *The Middle Kingdom* in 1848.

A key American value that he emphasizes in these essays is human rights. Many of the authors he reviews, as well as Fairbank himself, are critical

of the abuses of human rights in contemporary China, especially during and immediately following the Cultural Revolution. But he moderates his criticism by reminding the reader that "the human rights concept, though enshrined in a self-styled universal declaration, is culture bound" (p. 95). In China the concept of the individual has not had the same autonomous meaning that it has had in the West, and thus the country will never attain the same degree of openness and freedom that is taken for granted in the United States. Speaking of Chinese students now studying at American colleges and universities, for instance, he writes that their excellent academic performance "does not change the hard conditions they may face at work in China" (p. 209). He doubts that "the Americanization of China can go on unchecked," particularly in the realm of individual rights (p. 204).

In making such assertions, Fairbank is being true to what he has always insisted on, that is, developing a historical perspective on contemporary China so that current affairs may be examined in their proper context. This is undoubtedly important, but one wonders if it is sufficient to look at China's past alone. To appreciate a phenomenon such as human rights, would not one also have to consider the history of freedom in other lands, including Europe and the Middle East? How China-bound is China?

It is interesting to note that, whereas cultural contrasts form a core concept for Fairbank's view of China and Chinese-Western relations, many who have written on Japanese-Western relations have stressed the themes of convergence, affinities, identities, and the like, moving away from cultural determinism. Fairbank, of course, is not a crude cultural determinist, but with him the past seems to weigh heavily indeed. Paradoxically, this may be because China's past, especially its relations with the United States, has been so volatile and unpredictable. The cataclysmic changes in Chinese-American relations in the twentieth century caution one against facile generalizations, and it may be safer to predict nothing except the pervasive influence of cultural traditions. By the same token, however, there may yet be unforeseen changes even in the realm of human rights in China.

If, on this and other issues, "American minds" still cling to the "cultural values" in existence for two centuries, and if the Chinese, on their part, "are far and away the most history conscious" of peoples (p. 87), is there any way to develop a fresh perspective on their relations? Fairbank's suggestions sound modest at first. "Before we beat the drum of human rights in our China policy," he says, "we need to sort out global universals from

culture-bound particulars and find common ground" (p. 103). But to talk of separating "global" from "particular" values is to recognize the need to transcend a narrowly bilateral framework, that between China and the United States. He seems to be implying that one needs to make one's understanding of Chinese-American relations multilateral, to look at the cultures and traditions of other peoples as well as at those of the two countries. The need for internationalizing historical scholarship is as clear here as in other fields.

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SHELDON GARON. *The State and Labor in Modern Japan*. Berkeley and Los Angeles: University of California Press. 1987. Pp. xvi, 326. \$35.00.

In this study of the evolution of social policies toward labor in modern Japan, Sheldon Garon argues that Japan's labor history must be written as more than a movement from below. Despite a well-documented pattern of suppression of the labor movement, the state was not always or singularly opposed to organized labor. Exploring the complex relations among business groups, parties, factions of the labor movement, and bureaucratic cliques, the author finds no clear-cut battle line dividing capitalists and the state against the proletariat. Instead, he describes a complicated web of shifting and unpredictable alliances, as various elites tried numerous formulas for ensuring harmonious industrial relations. In this multifaceted process, Garon claims, the "initiative behind many of the most important democratizing reforms of the 1920s came not so much from the bourgeois parties—and certainly not from the weak social democratic movement—but rather from activist cliques of higher civil servants" (pp. 73–74). The strange bedfellows of Japan's modern labor history were the worker and the career bureaucrat.

On the face of it, this conclusion is not surprising. Many other reforms bore the stamp of Japan's highly educated, elite bureaucratic corps, whose members viewed their role—and that of the state—as one of mediating social conflict for the benefit of the polity as a whole. If their concern with the living and working conditions of laborers was not always inspired by humanist impulses or commitment to social justice, it was always driven by nationalist goals of promoting industrial productivity, preserving national unity and social order, and heading off radical socialism. The middle-level bureaucrats in the Social Bureau of the Home Ministry who are highlighted in this study, however, held ideals that, according to the author,

"were often 'liberal,' for they envisioned the optimal society as one in which interest groups freely and fairly competed before the referee-state" (p. 118).

Garon's biographical material convincingly bears out this interpretation of the Young Turks in the Social Bureau, at least as far as the decade of the 1920s is concerned. They produced a copious stream of proposals for broadening franchise rights, assuring workers' rights, liberalizing censorship provisions, and recognizing unions, and they fought with varying success to get these proposals passed into law. More problematic is the behavior of the social bureaucrats in the 1930s, when, in the midst of Japan's frenzied military build-up, they adopted stringent controls over both labor and capital, and, in their relentless quest for mechanisms to settle disputes, preserve social harmony, and increase productivity, they experimented with a variety of measures, including some inspired by Nazi Germany and Fascist Italy, before presiding over the dissolution of labor unions altogether. Although Garon sees these bureaucrats as "determined to establish a liberal, yet supervised framework within which unions would develop" (p. 237), other historians may be more inclined to explain their motivation and behavior in terms of a twofold strategy for preserving national solidarity by promoting social reforms on the one hand, while on the other hand continuing to promote the collectivist ethic.

In the postwar period, some of the same flexible and seemingly indefatigable bureaucrats helped draft and implement labor reforms during Japan's occupation, many of which resembled the draft proposals they had cranked out in the 1920s. They have continued to stand guard against leftist threats, but, in a departure from the prewar past, in the present day they seem more aligned with business and agriculture than with labor, as the Japanese state "pursues its paramount goal of economic growth" (p. 242).

Garon's study offers a rich appreciation for the interplay of forces shaping industrial relations over the past century. The picture is incomplete, because it lacks an explanation of the role that social bureaucrats, employers, and the workers themselves played in forging Japan's famed management style and establishing paternalistic practices to cope with problems of modern industrial society. Otherwise, Garon provides a solid historical background for comprehending relations between organized labor and government, and, by taking his account into present times, he ably demonstrates continuities with the past. He also scrupulously engages problems of interpretation every step of the way, clearly articulating his reasons for building on or parting company with

other scholars' views. The result is a gracefully written, absorbing work.

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KIMITADA MIWA. *Nihon: 1945—nen no shiten* [Japan: From the Viewpoint of 1945]. Tokyo: Tōkyō Daigaku Shuppankai. 1986. Pp. xi, 237.

Kimitada Miwa's message is that the postwar Japanese should rise above what Marius Jansen called the "exorcism of the irrationality of the recent past." This rejection of everything that went "wrong" in prewar Japan is an exercise that, as Miwa sees it, has led the Japanese to bury the old Japan in complete oblivion. Miwa asks what there might be in the old, aggressive Japan to be reassessed and, if possible, salvaged (pp. 164–65). His small yet highly provocative book is a promising start for such an inquiry.

The underlying issue of the book is the tension that emerged within the Japanese consciousness as Japan developed into an Asian imperialist power following the Sino-Japanese War—a "just" war for the emerging state, as Uchimura Kanzo saw it. This tension, which eventually silenced Uchimura, came from the split identity of Japan as a modern nation-state and an Asian nation in a region torn apart by the very type of modern state Japan was aspiring to become. As Miwa presents it, this tension only deepened as Japan's growth as a modern state quickly became the source of domestic and international strains in itself. First, the collusion of mercantilist interests among big business, the political party establishment, and the military belied what appeared to the Japanese to be progress toward a more open society during the euphoric Taisho era. Second, after the Russo-Japanese War, Japan could not elude close scrutiny by a suspicious West plagued by "Yellow Peril" hysteria.

Thus, Miwa posits an intriguing parallel between Japan's perceptions of itself domestically and internationally. He observes the increasing domestic grievances among the Japanese who had grown up in the seemingly liberal Taisho Japan and, simultaneously, their increasing awareness that the international society that Japan aspired to join was simply a forum for the Anglo-American monopoly of international affairs. The seeds for the fanaticism of the 1920s and 1930s were planted, Miwa seems to suggest, when the promises of an open society within and without were finally broken as Japan moved into the Showa era. Just as many Onuma Tadashis on the domestic front sought redress for the aggrieved Japanese in the direct political action of the 1920s and 1930s,

so many Konoe Fumimaro's, with their rejection of "the Anglo-American collusion of interests" in the international status quo, sought their own means of redress for an aggrieved Japan on the international front.

Hence, the stage was set for a Japan in the 1930s with an exclusionary ideal of Pan Asianism, which had never before figured prominently in Japan's national policy. This Pan Asianism manifested itself in several ways. Among others, there was Matsuoka Yosuke's infamous Greater East Asian Co-prosperity Sphere; there was the Pan Asianism of Nakayama Masaru, Prince Konoe's "ghost writer," which amounted to Sino-Japanese cooperation in resisting Western encroachment; and there was Royama Masamichi's conception of a Pan Asianism capable of coexisting with other blocs in the Pacific region. A common thread in all was the belief that Japan, having been aggrieved by the Western-dominated international order, was perfectly justified in speaking for other aggrieved nations, especially in East Asia, and defining their means of redress in terms of Japan's own choosing. The danger that many took lightly—too lightly—was a Japan treading the very path of the transgressors against whom Japan was representing the voices of all aggrieved.

There is a special voice throughout the book. This voice says, sometimes loudly and at other times almost inaudibly, that with or without Japan's Pan Asianism, the aggrieved remained aggrieved throughout East Asia. Miwa wants us to see that somewhere in the ideal and intellectual foundations of Japan's prewar Pan Asianism, with its self-determination of Asian peoples and the notion of regional community for economic cooperation, was the impulse to supersede the "Japaneseness" of Pan Asianism. In reconstructing those foundations, Miwa freely (sometimes too freely) moves among numerous prewar Japanese intellectuals and government officials. Familiar figures such as Nitobe Inazo, Yanaihara Tadao, and Royama Masamichi are everywhere. Not so familiar figures such as Tarui Tokichi are also given important roles. Miwa also runs quickly through a score of research associations these individuals used as their forums. Thus, the book offers a far richer sense of what it is built on than its relatively small size indicates. At the same time, I kept wishing for a bit more substance on some of these individuals, as well as for more powerful, and coherent, statements linking them.

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STEIN TØNNESSEN. 1946: *Déclenchement de la guerre d'Indochine; Les vèpres tonkinoises du 19 décembre.*

Translated by BRUNO METZ. (Recherches Asiatiques.) Paris: L'Harmattan, with the cooperation of the Conseil Norvégien de Recherches Scientifiques, Oslo. 1987. Pp. 275.

In the vast outpouring of materials on the Vietnam War, relatively little has recently appeared on the role of the French in instigating the conflict. Scholars still tend to rely on useful but essentially dated accounts, such as Philippe Devillers's *Histoire du Vietnam, 1940–1952* (1952), and Ellen J. Hammer's *The Struggle for Indochina, 1940–1955* (1954). Fortunately, the French government is now beginning to open its vast archives on the postwar period, thus opening up a rich source of potential new insights on the first years of the war.

This study is one of the first fruits of that process. Stein Tønnesson, a Norwegian historian, has used the French archives to undertake an exhaustive study on the brief interlude of negotiations between the Ho-Sainteny Agreement of March 1946 and the outbreak of the Franco-Vietminh conflict in December.

From the outset, those negotiations were plagued with difficulties, particularly in France, where leftist parties struggled against efforts by conservatives led by High Commissioner Thierry d'Argenlieu to scuttle the March agreement and restore colonial rule in Indochina. During negotiations held at Fontainebleau during the summer of 1946, the Vietnamese president, Ho Chi Minh, attempted to manipulate French political differences to his advantage. Ultimately he failed, and in September, in a desperate effort to prevent the breakdown of the negotiations process, Ho signed a *modus vivendi*, salvaging a cease-fire and postponing new talks until early the following year.

It was the issue of customs that eventually sparked conflict. Colonial officials in Saigon attempted to seize control of Vietnamese customs in the north in order to prevent the Vietnamese from exchanging rice exports for munitions. Their efforts led to the famous incident in which French naval units shelled the native city of Haiphong, killing thousands. Although efforts continued on both sides to head off the conflict, the Haiphong incident set the two nations on the road to war. Convinced that the French were preparing their own assault, the Vietminh launched a preemptive attack on key French installations in the Hanoi area on December 19, 1946.

Ultimately, Tønnesson's account leaves as many questions unanswered as it solves. Why did the Vietminh attack just when the appointment of the French Socialist leader Leon Blum as prime minister suggested the possibility of a peaceful resolution of the crisis? Was Ho Chi Minh's last-minute appeal to Paris deliberately delayed by officials in

Saigon in order to prevent a settlement? Finally, was the Vietminh attack on Hanoi carried out as planned, or, as the author suggests, was there a fatal moment of indecision among party leaders that was only resolved by militant elements (some of them connected with rival nationalist parties) among the militia?

Final answers to the "vespers of Tonkin," then, are still hidden in the French archives and in the minds of surviving party leaders in Hanoi. But Tønnesson has done scholars a distinct service in this taut and provocative account of the events that form a prelude to one of the most tragic periods in modern times.

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R. J. MOORE. *Making the New Commonwealth*. New York: Clarendon Press of Oxford University Press. 1987. Pp. xi, 218. \$48.00.

The independence and partition of the Indian subcontinent have fascinated historians since 1947 and no doubt will continue to do so for justifiable cause. They were complex events, possessed of both high drama and long-term significance. R. J. Moore himself has made important contributions to the literature, most notably in *The Crisis of Indian Unity, 1917–1940* (1974) and *Churchill, Cripps, and India, 1939–1945* (1979). In *Making the New Commonwealth*, he focuses more precisely on an aspect that has not always caught the attention it deserves. The issue, put too simply, was whether India, Pakistan, both, or neither would continue to have a special relationship with Britain and the Dominions within the Commonwealth.

The issue arose not merely from the deep anger over the form or costs of partition, from frustration over specifics like Kashmir or Hyderabad, or from the truism that, were Britain to risk choosing to favor Hindu or Muslim, it could be sure of alienating one of them. All of this was trouble enough to jeopardize future relationships. Less immediately obvious, but clear to any reflective observer, was the fact that the Commonwealth was a club of white societies, and the idea of opening it to "other races" in the aftermath of World War II was as traumatic in its way as that of opening the local cantonment club to Indian civilian or military officers had always been. Many across the world could accept the principle, but applying the practice was something quite different, most notably where Dominions had racially exclusive immigration restrictions. That particular obstacle could be overcome in the end by dropping all ideas of common citizenship, along with suggestions of common defense arrangements. It was less easy to

find a satisfactory rationalization for Burma's quick and final pronouncement that it was uninterested in any such special relationship. Since Eire, too, was moving away from Commonwealth status, the portents were ominous.

All of this Moore explains well in this short but fully annotated and well-written study. Treated in less detail, inevitably, are the two other contexts in which decisions to reshape the Commonwealth were made. The first was that of the Middle East, where Britain was struggling to preserve Arab friendship in the midst of an insoluble dilemma in Palestine. Pakistan was Muslim and, to some, a necessary bulwark of Middle Eastern stability. Indian leaders, fully aware of Britain's concern, were inclined to see decisions favoring Pakistan as just that, favors given for services rendered and not awards made on the merits of the case.

The other context was that of the cold war, and it led to the gravest of inter-British disputes. Ernest Bevin, under the urging of his Foreign Office advisers, argued that India within the Commonwealth would simply pressure other members to pursue India's policies and larger vision of the future of the Asian world. Alternatively India would leave the group, and either eventuality would weaken the institution, perhaps irretrievably. Not without major struggle was Bevin converted to the larger view that India outside the Commonwealth—aside from forcing the group to back Pakistan in every quarrel—would be far more susceptible to Communist influence, jeopardizing Britain's entire position in Southeast Asia and the Far East. Once past this debate, one final issue, format, remained. India was a republic, not a monarchy, and was unwilling to give homage to the king as king. It was left to Sir Girja Bajpai, a "patrician Anglophile" (p. 201), to find a formula with the simplicity of genius: the king would be the symbol of free association only, "Head of the Commonwealth," not its ruler.

To these, and other issues, Moore brings economy of style, enviable expertise in the sources, and years of reflection. The result is an admirable book. It would be easy to ask that this or that theme (particularly concerning extra-India influences) be developed more fully, but such additions would demand a different, and still more expensive, book. The plan Moore has adopted works well. This volume belongs on the shelf of anyone seriously interested in twentieth-century India and in the significant stages of the evolution of the Commonwealth as a whole.

BRITON C. BUSCH
Colgate University

MICHAEL ROE. *Nine Australian Progressives: Vitalism in Bourgeois Social Thought, 1890–1960*. (University of Queensland Press Scholar's Library.) Lawrence, Mass.: University of Queensland Press. 1984. Pp. 328. \$37.50.

After an introductory essay on Australian progressive thought as "vitalism," Michael Roe focuses in his biographies on activism from the 1890s to the 1930s—what Michel Foucault has called "bio-politics," that is, the complex of policies (industrial, municipal, educational, medical) managing the social reproduction of populations. Roe's genre, prompted partly by his admiration for his subjects, also shows the links between careers and institutional growth, but he declines to make general conclusions about that linkage.

W. J. Brown was a jurist who made his mark in the expanding field of industrial arbitration and also espoused eugenic prescriptions. For Roe, Brown embodied the progressive paradigm. J. W. Barrett, physician, concerned himself with the organization of the work of health professionals but created a crowded agenda that included spelling reform. His last speech, a warning against "neo-phobia," was "pure progressivism" (p. 84). J. S. C. Elkington introduced medical inspection into Tasmanian schools, arguing that "education is a biological process whereby certain discrete growing organisms are in process of being fitted to their environment in order to render them more useful to that concrete organism, the State" (p. 99). J. H. L. Cumpston, founding director general of the Commonwealth Department of Health (1921–45), dealt with medical problems in the settlement of tropical Australia and in other ways exemplified the interest of progressives in the strenuous enjoyment of nature. Richard Arthur, another physician, tackled issues such as immigration and defense as part of a holistic consideration of the fate of Anglo-Saxons in the Antipodes, advocating state child-rearing subsidies for parents. G. A. Taylor, editor and publisher, contributed to Australian building and architecture an enthusiasm for modern materials and methods. In his journal *Soldier*, he extolled war as a catalyst of the world's technological destiny. A. B. Piddington, lawyer, social planner, and economist, sought amelioration of the poor living conditions of working-class families. His influence depended on the thrust of the Labour Movement's reforming impetus in New South Wales (NSW). He fared better than R. F. Irvine, the Sydney University economist whose views paralleled the ideas of J. A. Hobson and J. M. Keynes and who counseled NSW Labour governments on redistributive housing and social policies, warning of the social disasters that inequalities of wealth would bring. Aspects of his

sex life gave his enemies the excuse to demand his resignation in 1922. E. M. Miller, writing on philosophy, library administration, the measurement of mental ability, and mental hygiene, influenced Tasmanian legislation and administration.

Roe's "vitalism" is too inclusive to clarify the themes of these men's progressivism. But a common political philosophy does emerge. It championed the common good against "interests" and looked to a strong state directed by outstanding leaders and guided by experts. It manifested ambivalence about democracy, professed belief in racial destiny through national endeavor, and displayed a passion for nature and efficiency. The Great War vividly showed to each person modernity's best and worst potentials. The institutions these experts guided increased state intervention into public health and family-centered policies.

Roe sees progressivism and feminism as distinct and even antagonistic. Yet occasionally he refers to women who espoused simultaneously the interests of women and of Anglo-Saxon race efficiency. Gender-centered research now elucidates these contingent features of early twentieth-century Australian feminism. Such work implicitly contests Roe's choice to write a history of Australian "bio-politics" as a series of biographies of men. Nonetheless, his researched and terse monograph will be an essential starting point for those inspired by Foucauldian and feminist insights to reexamine the implications, for gender relations and for subject populations, of progressivism's Australian episode.

TIM ROWSE
University of Sydney

BEDE NAIRN. *The "Big Fella": Jack Lang and the Australian Labor Party, 1891–1949*. Melbourne: Melbourne University Press; distributed by International Specialized Book Services, Portland, Oreg. 1986. Pp. xiv, 369. \$35.00.

John Thomas Lang (1876–1975), born in Sydney, the son of immigrants from the United Kingdom, was a tough leader of the faction-ridden, brawling Labor party in New South Wales (NSW), Australia's most populous state, from July 1923 until September 1939. He was premier from June 1925 until October 1927 and from November 1930 until May 1932. A child of the slums, Lang became one of Australia's best-known politicians, arousing intense passions among supporters and opponents. The "Big Fella" was an appropriate sobriquet, for he weighed about 214 pounds and was six feet four inches tall. "A human thundercloud," a humorless man without real friends and with a grudge against society (despite making money out

of a real estate business), he was a populist demagogue who nevertheless conformed to the conventions of the British Westminster system of government. He was a Catholic, infrequently attending church, and implacably anti-Communist. His most unattractive qualities were partly redeemed by his personal honesty and his sponsorship of some minor legislation to improve the lot of society's underprivileged members.

Although quasi-autobiographical works, and many brief articles, have been written about Lang, this is the first substantial scholarly study. In a skillful, thoroughly professional work, Bede Nairn presents a completely satisfying analysis of his subject's life and political career: his rise to power, his relatively unproductive first premiership, his part in Labor's factional brawls, and his power struggle with E. G. Theodore, a prominent Labor man holding a seat in federal parliament for an area close to Lang's home base in Sydney.

By November 1930 Lang was premier again, involved in bitter disputes with a year-old federal Labor government—Theodore its treasurer—over financial policy to counter the Great Depression. Nairn lucidly describes the steps leading to the formal breach in March 1931 between the state and federal Labor parties and the subsequent financial and political crisis in NSW in May 1932, which was resolved by the state governor (an Englishman, Sir Philip Game, appointed as His Majesty's representative), who dismissed Lang from office because he was acting illegally. Lang was thrashed in the ensuing election, never again held ministerial rank, and lost the party leadership in a turbulent coup in 1939. Thereafter he had only a minor role in politics, although he was admired by industrial workers and, later, by youthful idealists and others who took his rhetoric at face value and saw him as defending the deserving Australian poor against the wickedness of their class enemies, the rich, in Australia and Britain.

Lang was egotistical and virtually unread, although intelligent. His greatest joy in life was politics, yet he achieved little constructive as a politician by way of legislation, although he had a great, largely destructive, impact on the whole of the Labor party. His attempts to control the depression "were reckless and finally disastrous" (p. 316).

Nairn has succeeded brilliantly in piecing together a readable, clearly argued, detailed, and authoritative account of some intricate internecine political warfare. This is by far the most useful book on NSW politics of the period.

JOHN ROBERTSON
University of New South Wales

MARILYN LAKE. *The Limits of Hope: Soldier Settlement in Victoria, 1915–38*. New York: Oxford University Press. 1987. Pp. xviii, 317. \$37.00.

The nature and the causes of failure of the attempt to settle large numbers of Australian ex-servicemen on farms after World War I has been the subject of a number of government enquiries, but a full description of why and how it was undertaken and its results has never been published. *The Limits of Hope* is an attempt to examine the settlement scheme in the state of Victoria. As Marilyn Lake points out, the major causes of failure were that some men were suffering from war injuries, many had little farming experience, and the areas of settlement were only marginally suitable. Most of the farms were too small to support a family, and the settlers were supplied with insufficient capital to make their land productive. The unexpected decline in agricultural produce prices in the post-World War I period made their position even worse.

Lake also discusses some of the less well known aspects of the scheme. For example, the state authorities recognized that the plan would not succeed before it was commenced, but they continued it as a means of demonstrating that something was being done for the first veterans when they returned in 1915 in order to prevent them from interfering with army recruiting during the remainder of the war and to encourage more men to volunteer. Control by the settlement authority of the revenue from sales of settlers' produce, parsimonious expenditure by the state on both farm requisites and household goods, and the personal behavior of the authorities, together with the humiliation associated with state control, are also discussed in detail.

The work is strengthened by a sample drawn from three hundred files from the ten thousand five hundred ex-servicemen who settled in Victoria. The use of the correspondence between the settlers and the settlement board in these files presents the human aspect of the scheme, particularly its effect on the settlers' families, even though the use of this material possibly overemphasizes the position of the least successful settlers as opposed to those who were more successful.

It is doubtful that Lake completely substantiates her contention that the scheme was essentially one of settling yeomen farmers who relied on family labor as opposed to the settlement scheme after the Second World War of settling capitalist farmers employing labor. The settlement board objected to the employment of labor not because of ideology but because it increased the settlers' indebtedness. The authority's objection to other forms of expenditure appears to have been just as

great. The settlement scheme at the end of the Second World War was successful because it was limited to experienced men who were settled on farms in favorable farming areas, large enough to support a family, and were supplied with adequate capital to develop their farms. In addition, the prices of primary produce remained high. Few settlers in the post-World War II scheme were employers of labor.

The book is very readable and must be regarded as a major contribution to the literature dealing with land settlement in Australia.

BRUCE R. DAVIDSON
University of Sydney

L. DE JONG. *Het Koninkrijk der Nederlanden in de Tweede Wereldoorlog*. Volume 11c, *Nederlands-Indië III* [The Kingdom of the Netherlands in the Second World War. Volume 11c, Netherlands East Indies III]. Leiden: Martinus Nijhoff. 1986. Pp. vii, 751.

Volume 11 (in three books) of L. de Jong's epic account of the Netherlands during World War II deals with the Netherlands East Indies from the eve of the war in Asia to the return of Dutch authority to Batavia (Jakarta), 1940–45. The third book of this volume examines the activity of the Netherlands government-in-exile in so far as it relates to the East Indian colony. Queen Wilhelmina and the cabinet in London laid down general guidelines for future colonial relations, which were made public in the queen's address of December 6, 1942. The speech had been consciously scheduled on the eve of the anniversary of the bombing of Pearl Harbor, for the queen's message was meant as much for the American public as it was for the Dutch and Indonesian peoples. Her assurance that after the war the kingdom of the Netherlands would be structured on shared participation by its various components satisfied no one: American opinion found it too little, the Dutch too much, and Indonesians (in so far as they knew about it) wanted autonomy, as Soejono, the only Indonesian in the London cabinet, eloquently but unsuccessfully urged upon his colleagues. Instead the views of H. J. van Mook prevailed. His public relations efforts with American officials did not do much to change the American anticolonial bias.

The activities of the Netherlands Indies government-in-exile took place in two theaters of war until shortly before the end of the Pacific campaign. Sumatra fell into Lord Mountbatten's South-East Asia Command, and the rest of the colony fell into Douglas MacArthur's Southwest Pacific Command; in this way Dutch forces and

actions were divided between Ceylon and Australia. To have their limited capacity thus divided, to be a minor ally having to beg for materiel, and to trim their operations to fit the patterns of the major partners were all extremely frustrating for the Dutch. Over and beyond these uncontrollable matters, however, the Dutch managed further to complicate their wartime endeavors by personal distrust and bickering, awkward divisions of responsibility and authority, and downright stupidities that make this narrative read like a tragicomedy.

The gathering of information about Japanese policies and strengths in the East Indies was of primary importance to the war effort. Yet by the end of 1943 every effort in this direction resulted only in the deaths of brave men; no information was gained except for the New Guinea area. This failure must be blamed on the ineptitude of those responsible for these operations. In the last year and a half of the war a few shreds of information were obtained but mostly from fringe areas; nothing was learned about the island of Java. What information did come to the Dutch through monitored broadcasts and newspapers was tailored to fit preconceived notions of Indonesian character held by Dutch officials at the highest level. The result of this bias can be seen in Lieutenant Governor General van Mook's announcement to the Dutch in Australia on August 14, 1945: "We shall find millions of Indonesians who, as we know, have taken their stand in overwhelming masses against the enemy and stand totally on our side" (p. 509). Only three days later the Republic of Indonesia was proclaimed.

This tragedy of useless losses and misinformation is balanced by a series of comical blunders by the Dutch in Australia. Dutch refugees from Java retained their colonial pay scale, thereby putting their affluence in sharp contrast to the austerity of their hosts. Indonesian seamen, on the other hand, were kept at their substandard wage. Soon they and Indonesian prisoners, who had been evacuated to Australia from New Guinea, were assisted by the Australian Communist party to set up an Indonesian Communist party complete with front organizations. The Dutch, apparently oblivious of this, used these Indonesians in their reoccupation of the archipelago. Despite the promises contained in the queen's speech, most of the Dutch in Australia had no intention of lowering the colonial racial barriers, which resulted in minimal communication between Hollanders and Indonesians.

De Jong's account makes evident the gulf that separates the values and viewpoints of the 1940s from those of the 1980s; he gives great credit to individuals but points openly to the smallness of

spirit and vision of the Dutch wartime effort in Asia.

ROBERT VAN NIEL
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UNITED STATES

HOWARD GILLETTE, JR., and ZANE L. MILLER, editors.
American Urbanism: A Historiographical Overview. (Contributions in American History, number 125.) New York: Greenwood. 1987. Pp. vi, 328. \$45.00.

Twice in this century, urban history has offered the promise of renovating American history. In *The Rise of the City* (1933), Arthur Meier Schlesinger, Sr., meant to document the positive, dominant role of the city and contrast it with Frederick Jackson Turner's emphasis on the frontier. Schlesinger's initiative was followed by publication of the numerous urban biographies his students wrote to prove their mentor's point. In the 1960s, a "new urban history" emerged that owed little to Schlesinger. It was not meant to document the city's contribution to progress. Rather, it was prompted by the need to trace the origins of social ills so visible in the modern American city. Sam Bass Warner's *The Urban Wilderness* (1972) came closest to capturing the goals of the new urban history as Schlesinger's *Rise of the City* had embodied those of the old.

Now, according to editors Howard Gillette, Jr., and Zane L. Miller, the field is in transition. Both agendas have been fulfilled, and, as a result, some scholars have even questioned the future of urban history. Therefore, the time has come to review the results of the abundant scholarship of the last twenty-five years, hence, this modest, historiographical book, with abundant footnotes and two indexes. Gillette and Miller do not pretend to be setting a new agenda; rather, they wish to take stock of what has been done.

Most useful are the surveys of subfields still unfamiliar to many historians, such as the review essays on planning history (Eugenie Birch), architecture (Richard Longstreth), and engineering (Joel Tarr and Josef Konvitz). Equally useful are two good surveys of historical geography, one on land use within cities (Edward Muller) and one on the region and patterns of metropolitan dominance (Carl Abbott). The bulk of the volume covers more familiar territory and brings us up to date with the literature on attitudes toward the city (Gillette), race and ethnicity (Andrea Tuttle Kornbluh), work (Leonard Wallock), urban governments (Jon C. Teaford), and suburbanization (with two separate essays by Michael H. Ebner and Carol A. O'Connor).

Least useful are the essays that are conceptually more ambitious than those on subfields, such as a poorly focused chapter on the city as a social system (Alan Marcus) that seems to argue that urban historians are too preoccupied with the present moment and a meandering essay on neighborhoods (Patricia Mooney Melvin) that bypasses much important work in political history and does not offer any conceptual breakthroughs. The final essay on local history (Robert Dykstra and William Silag) surveys monographs of small communities only and treats local history as if it were a recent creation. It would have been instructive to contrast the new, methodologically sophisticated urban monograph to the old urban biography.

The final impression that this book leaves is that urban history has remained unabashedly local. Activity in urban history has been focused on community studies, investigations of local politics, analyses of local expressions of technological and engineering innovations, studies of segregation patterns in local real estate and housing developments. The authors succeed in showing that these specialized studies have yielded important results. The continued vitality of urban history, however, depends on the ways in which historians of localities relate the particular to the general, for the history of a "place" leads inevitably to a reappraisal of the broader framework that constitutes intellectual and historiographical debate. On this larger topic, the volume provides little guidance.

OLIVIER ZUNZ
University of Virginia

JAMES L. MACHOR. *Pastoral Cities: Urban Ideals and the Symbolic Landscape of America*. (History of American Thought and Culture.) Madison: University of Wisconsin Press. 1987. Pp. xvi, 272. Cloth \$45.00, paper \$12.95.

Although his title suggests (to me) that this is a book about real cities and real landscapes, in fact James L. Machor offers us a rereading of the classic literature on the idea of the New World, from the age of discovery to the age of Nathaniel Hawthorne, Walt Whitman, and Daniel Drake, with a brief and busy postscript on later writers and our contemporary debate on the quality of urban life. The point of this rereading, parts of which have been previously published, is to discover the origins and trace the development of "urban pastoralism," which Machor argues is a central theme in American culture and a critical element in the definition of American uniqueness.

Machor is uncomfortable with the notion that America is "nature's nation," but he also rejects the

counterarguments of those who have seen America as a nation of cities. Both positions are reductionist and therefore inappropriate ways of describing the real complexity of American culture, and both talk a static language of ideas in tension, inappropriate to the dynamic reality that has characterized American history. What is needed, he says, is not more discussion of the ways Americans sought to resolve these tensions, by "nature-izing" the urban experience or "civilizing" nature, for example, but some new model altogether.

It is true, Machor says, that nature was viewed as natural and cities as artificial, and, for those who viewed the natural as good and the artificial as bad, nature was therefore good and cities were therefore bad. But it is also true, he says, that a tradition existed of viewing the city as a model of community and of locating the "ideal" city as an essential element in an idealized Peaceable Kingdom. This is "urban pastoralism." Derived from Judeo-Christian iconography, it flourished in late medieval Europe and entered America with the Puritans.

"Before 1520, . . . the absence of a providential context severely undermined the credibility of urban-pastoral projections upon the new land" (p. 33). By 1620, however, "America had been conceptually transformed" into "a pathway, charted by Providence, to a future society uniting the advantages of urban, communal existence with the bounty of a domesticated natural paradise" (p. 44). In the eighteenth century, urban pastoralism itself became transformed "from an essentially ahistorical accomplishment of Providence to a secular, historical possibility" (p. 71). As the possibilities of the New World were redefined "as a pathway to origins" rather than as "a continuation of history" (p. 121), urban pastoralism emerged as an essential element in the idea of America, manifest in the conventional descriptions of particular cities "as arising, like agriculture, from natural laws and the distinctive features of the native terrain" (p. 123), and especially in the ambivalence of American writers, who could not decide which was wilder, the city or the country, and which was more civilized.

Machor's readings of the classic texts are ingenious and always helpful, but I am not persuaded that naming this familiar ambivalence "urban pastoralism" effectively legitimates it as a discrete element in the history of American ideas or advances our understanding of how we have dealt with it in the past—and will in the future.

HENRY D. SHAPIRO
University of Cincinnati

DAVID MARR. *American Worlds since Emerson*. Amherst: University of Massachusetts Press. 1988. Pp. x, 234. \$22.50.

David Marr agrees with those Boston bankers who, according to Ralph Waldo Emerson, interpreted transcendentalism as threatening the sanctity of contracts. Marr is concerned with the political-social contract and in this study claims that Emerson's "privatism, moralism, and anti-politicism" established an American tradition that has sanctioned "the reckless attack on the very idea of public life" (p. 4), so that little remains but "fragmented sub-specialties of knowledge which rationalize modern modes of social control" (p. 172).

Marr's concern is to rescue a few American writers from literary-historical scholars who suffer from a terminal "poverty of philosophic ideas" (p. 7) and to analyze some bit of their work to help stave off the deepening shadow over American democracy. Some of Marr's exegesis is sharply sensitive, especially for figures whom he respects such as Emerson, Whitman, R. P. Blackmur, and Ralph Ellison. Yet there is a random quality to Marr's choice of subjects, which also includes William James, Robinson Jeffers (much maligned), Joseph Heller, and Margaret Atwood, imported southward into this American tradition seemingly as a last-minute gesture to feminism.

The cement of the book is not a close linking of these writers but a political stance, the positive aspects of which remain hazy. Marr dislikes instrumentalism, liberalism, bourgeois individualism, racism, sexism, scholarly subdivisions, and those who want to keep *Huckleberry Finn* out of the public schools. He likes democracy, which represents "the only chance of averting the final reification of the automatonicity that flourishes when recognition fails" (p. 214). He also likes words like automatonicity, facticity, and entelechy, which sometimes contributes to a sense of inflated vagueness. Yet Marr argues that democracy, defined as "full participation in self-government," has never existed in the nation and has deteriorated in the twentieth century (p. 5). And does the use of "self-government" suggest commitment to the public or the enshrinement of the private that Marr ostensibly attacks?

Part of the book's oddity is that it seems to offer not a political answer but the kind of personal exhortation that it criticizes. Marr is surely right that Americans often see or seek political solution in the realm of the private, the aesthetic, or the social, with educators, ministers, and reformers (along with his literary people) promising social cure through character change formed and informed by their chosen nostrum. Yet, if Whitman's hope that the nation was to be saved by

poets (who were to become "sweet democratic despots of the west") seems quaint, salvation through literary philosophy appears at least as unlikely.

For all this thematic elusiveness, the book often shows a quietly revealing intelligence and care that well represent recent attempts of philosophers to deal with literary-historical questions. The history here consists of only the broadest generalities of Heidegger, Horkheimer, and Arendt about the transition from the medieval or Wittgenstein and Charles Newman on change in the twentieth century, but Marr makes a serious and provocative effort to grasp an American dilemma. Much like Stanley Cavell, who clearly influences the book, Marr shows intriguing insight somewhat blurred by insufficiently coherent and close development. That the whole of the book seems less than its parts does not destroy its value, especially for the groups it criticizes, Americans and historians.

DAVID GRIMSTED
University of Maryland

JOHN MOORE. *The Cheyenne Nation: A Social and Demographic History*. Lincoln: University of Nebraska Press. 1987. Pp. xxv, 390. \$32.50.

After the monumental work of Donald J. Berthrong, one might wonder what need there is for another history of the Cheyenne Indian tribe. The answer is that, despite its subtitle, John Moore's book is not history in the commonly accepted sense of the word, that is, a chronological narrative of events. It is ethnology, as such chapter titles as "The Tribal Circle," "Patterns of Marriage," and "Cheyenne Kinship" reveal, and any chronological sequence must be worked out by the reader from the wealth of data provided.

Moore's purpose is to use the Cheyennes as a case study to support a theory of the rise and fall of nations. At the risk of some oversimplification, the burden of his message may be summarized briefly. Tribal nations, like nation-states, originate in response to inner tensions, and their formation constitutes an attempt to solve perceived problems. Ethnically heterogeneous, they contain the seeds of their own dissolution, despite efforts to achieve homogeneity. The Cheyenne nation came into being about 1740, when four bands sharing common cultural traits united under the leadership of Sweet Medicine, who created a national "charter." A century later a dissident faction, the military society called the Dog Soldiers, seemed on the way to becoming a separate nation, but its defeat by U.S. troops in 1869 arrested this development.

Moore takes issue with traditional interpreta-

tions of the Cheyenne experience, sometimes convincingly, sometimes not. He is on solid ground when he emphasizes the dynamic nature of Cheyenne society and debunks the notion, fostered by early writers, that it, or any other society, was static, its institutions unchanged "from time immemorial." He is probably also right when he insists that war was a more serious business than George Grinnell and other early students of the Cheyennes would have us believe. He is less persuasive, however, when he argues that two tribal divisions reported by Jonathan Carver and traditionally regarded as Sioux subtribes were actually proto-Cheyenne bands. If he makes the orthodox association with the Sioux seem less certain, he does not overcome all objections to his own theory.

Moore devotes a great deal of space to methodology, explaining, for example, how the 1900 census schedules can be used to establish the locations of individual bands as early as the late eighteenth century. These explanations would be more justified if they clearly provided necessary support for conclusions. But sometimes they only substantiate conclusions already established by other evidence, and sometimes the methods seem to lead to ambiguous conclusions. Moore wants to impress his reader with the fact that he is using scientific data and to distance himself from "humanists" who disdain evidence and "most often appeal to prejudice or sentiment" (p. 17) when making their generalizations. There is a great deal of such airing of personal opinion, as when he berates historians who fail to do field work and anthropologists who neglect library research—two species of scholars who may be less numerous today than Moore supposes.

These are incidental faults in a book whose virtues far outweigh them. Moore writes well, and he does not overwhelm the reader with technical language. Even his chapter on kinship—a subject that he says separates the work of "amateur Indian buffs" from that of "serious scholars" (p. 287)—is readable. If some of his conclusions must for the present be labeled "not proved," he has performed a service by challenging traditional ways of looking at Indian cultures in general and Cheyenne culture in particular.

ROY W. MEYER
Mankato State University

FRANCIS JENNINGS. *Empire of Fortune: Crowns, Colonies, and Tribes in the Seven Years War in America*. New York: W. W. Norton. 1988. Pp. xxiv, 520. \$27.50.

This book completes Francis Jennings's three-volume campaign to write early American history

as it was experienced, with Indian relations at its center. In this volume, he focuses on the Seven Years' War, which began with the breakdown of the Covenant Chain that had linked the Iroquois League, its client tribes, and several colonies. The Albany Congress and Franklin's Plan of Union thus marked an end rather than a beginning. The system was breaking down as Covenant Chain members moved into lands beyond the Appalachians away from settlers' encroachment and away from the league's control.

The situation was extraordinarily complicated; within each colony different interests vied to control policy. For Indians the situation was infinitely more difficult. Jennings rejects use of the term "*the Indians*," with its implication of unified goals and problems. He illustrates the diversity of goals that characterized American relations and stresses that realization of the aims of some meant violation of those of others. This complex book amply proves Jennings's dictum: "*Nothing* was simple in frontier politics" (p. 80).

The Seven Years' War resulted from a new imperial policy, involving direct control of Indian affairs and all-out war with France for American empire. The American colonies were forced to pay for a war they correctly concluded was for England's imperial goals, and the campaign was conducted with arrogant disregard for American realities. The roots of the American revolution are firmly in the Seven Years' War, according to Jennings.

Indians participated in the war not as adjuncts of colonial powers but as independents with their own war aims. Many had hoped to "leave the Whites to fight" (p. 417). Their one overwhelming goal was preservation of a homeland for Indians already displaced from land east of the Appalachians. Repeatedly both imperial and colonial officials made promises. When these were broken, Indians attacked the frontier to try to force recognition of their claims. Forty years of warfare, including the misnamed Pontiac's Rebellion, ended only with the Treaty of Greenville in 1795.

Jennings does not treat the Seven Years' War in terms of large geopolitical forces. He is little interested in the underlying goals, movements, or trends by which most historians explain events. He sees history moving through the decisions of individuals who maneuvered themselves into positions of power, and he usually does not like what he sees. The intricate and confusing American scene offered ample opportunity to greedy opportunists on every level to manipulate situations for their own benefit. It is these manipulations, wrought by everyone from Thomas Penn, proprietor of Pennsylvania, Sir William Johnson, superintendent of Indian affairs, and Benjamin Franklin to the humblest frontier trader, that Jennings seeks to un-

mask. His indignation is as fresh as if the events occurred yesterday.

The heroes of his story are the Pennsylvania Quakers. Jennings argues that their peace plan was realistic and that the Quakers, far from being obstructionist, consistently worked to forge policy and practice that would achieve a lasting settlement. Their role has been distorted because historians, according to Jennings, have ignored Quaker sources and have used the manifestly distorted records of Penn and his associates.

Another sort of indignation is reserved for the military officers who almost uniformly conducted a self-serving war marked by bigotry and stupidity—from George Washington, whose defeat at Fort Mifflin came partly because, as Iroquois League spokesman Tanaghrisson put it, Washington "would by no means take Advice from the Indians" (p. 67), to Generals Abercromby, Amherst, Braddock, Loudoun, and Wolfe. The stupidity and brutality with which the war was conducted, and the British military tradition that allowed them, draw Jennings's scorn.

Jennings also castigates historians who have celebrated the manipulation and brutality and the imperialistic and militaristic assumptions underlying such celebration. Francis Parkman, founder of the still-powerful romantic myth of the colonists' struggle against Roman Catholicism and savagery and the subject of a recent debunking article by Jennings (*William and Mary Quarterly*, 3d. ser., 42 [1985]: 305–28), is presented as "stupidly vicious" and "a liar" (p. 126 n. 480). Others are criticized in their turns. Many of the footnotes in the book include separate bibliographical notes in which historians with whom Jennings disagrees are taken to task for misuse of sources or simply slashed for writing "slanted trash" (p. 84).

KAREN ORDAHL KUPPERMAN
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OMER C. STEWART. *Peyote Religion: A History*. (The Civilization of the American Indian Series, number 181.) Norman: University of Oklahoma Press. 1987. Pp. xvii, 454. \$29.95.

Omer C. Stewart is an anthropologist who has studied various aspects of peyote religion for the past fifty years. This volume is the culmination of his labors, and it will stand as the definitive treatment of its kind in our generation. More descriptive than interpretive, the work traces diffusion of peyote use from Mexico through most western states, from plains Comanche to desert Navajo, the northern Cree, and the Yakima along Puget Sound. It is the record of a unique and unifying phenomenon among postcontact American Indi-

ans, an indigenous movement through which they have adjusted to an America dominated by whites, at their own pace and in their own way.

The book's deceptively simple structure begins with the plant itself and shows peyote to have been in use for perhaps ten thousand years. Distinguishing it from mescal, with which peyote is often confused in documents, Stewart locates with helpful maps its natural habitat in northern Mexico and the Rio Grande Valley. The bulk of the book traces its spread among tribes on western reservations. In each painstakingly idiographic case he notes the personal influence of Indian missionaries on attitudes about the sacred plant, the admixture of Christian symbols, and variations in worship ceremonies. He also notes many constants in the religion, for example, that its initial appeal was always associated with curing. And general knowledge of peyotism always preceded practice; peyotists usually participated in other religions in addition to using the cactus; the religion always provoked opposition; and it has remained a minority religion in most tribes.

The narrative content in this work exhibits a dry economy of words. Pages often reproduce primary source material to substantiate a point, and generalized comment is severely restrained. The volume thus functions as a reference tool as well as a sequential story. Still, whether dealing with Quannah Parker in the early days, Sam Lone Bear in the erratic diffusion process, or Frank Takes Gun of the Native American Church of North America, whether crosslisting 305 ritual features of the standard ceremony in either the Big Moon-Cross Fire Way or the Half Moon-Tipi Way, this omnibus compendium on peyote religion is a valuable summation. It synthesizes previous literary sources and supplements them with ethnographic interviews and participant observations. It is the best and probably the only book most of us will hereafter need to consult.

HENRY WARNER BOWDEN
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MARILYN J. WESTERKAMP. *Triumph of the Laity: Scots-Irish Piety and the Great Awakening, 1625-1760*. New York: Oxford University Press. 1988. Pp. ix, 266. \$29.95.

Neither the title nor the subtitle of this work quite describes its subject. Marilyn J. Westerkamp seeks to document a primitive Presbyterian revivalist tradition that originated in seventeenth-century Ulster, appeared spasmodically in subsequent Irish and Scottish religious history, and erupted once more in the immigrant congregations of the Middle Colonies before the coming of George

Whitefield. She argues that this tradition is to be found in continuing rituals of mass conversion rather than in theological or institutional developments and that these rituals were cherished chiefly by the laity, often despite the discomfort or even hostility of the clergy. Thus, the Great Awakening among many Presbyterians is not to be viewed primarily as a novel, or even American, phenomenon but rather as the culmination of a popular Hiberno-Scottish lay religiosity.

The relation of immigrant Presbyterians to the Awakening has long continued to trouble historians. In his classic study, *The Forming of an American Tradition* (1949), Leonard J. Trinterud portrayed Scottish and Irish clerics as Old Side proponents of education, Westminster Confession, and organized structure (although he also acknowledged precedents of revival preaching in Old World Presbyterianism), but he made no attempt to explore these values in their British background. More recently, the transatlantic perspective has been enlisted, most relevantly in Elizabeth Nybakken's revisionist analysis of the "liberal" nature of Ulster Presbyterianism (*Journal of American History*, 68 [1982]: 813-32). Westerkamp now exploits this same transatlantic approach to establish a native revivalist potential in Scottish and Irish colonial laity.

Almost two-thirds of the text narrates British backgrounds. Revival practices began among Scottish planters in Antrim about 1625 and then spread to southwestern Scotland. "Irish innovations" included prayer meetings led by the laity, preaching sessions lasting three to five days, and emotional, sometimes convulsionary, lay responses. Of central importance was the immense protracted communion service as an effective ritual for achieving mass conversion. Westerkamp finds this revival tradition reemerging in Covenanter field-conventicles, in the Ulster Presbyterians' insistence on creedal subscription in the 1720s, and in Scottish Seceders' protests against patronage in the 1730s. Only the final seventy-seven pages of the book deal with the American Awakening where the Tennents again used the inherited communion ritual to further the New Light revival.

This study is an important addition to revisionist historiography on the Great Awakening, especially because it makes new and effective use of manuscript presbytery records at Belfast and Edinburgh. On the debit side are a few minor inaccuracies and an Irish map (p. 18) that fails to locate many places named in the text. Omissions and a tendency to claim too much are the book's graver problems. Despite the subtitle, Presbyterian piety is not adequately analyzed, nor is it clear why revivals first arose in Ulster (or failed to materialize there in the eighteenth-century revival). Indeed, Irish and Scottish churches are

often treated too indiscriminately without sufficient recognition of the unique problems of the Irish environment—a lawless frontier, a hostile establishment, religious diversity, and the necessity of voluntary organization. Strangely, millennial anticipations in both Scotland and America are ignored. In later chapters too much space is devoted to general background, thereby obscuring continuity of the revivalist tradition. That tradition itself rests partly on a perceived kinship of such “rituals” as subscription and covenant renewal with revival practices, although the subscription movements had primary objectives other than the harvest of souls. Even the title’s theme of “triumph of the laity” seems somewhat forced, since evidence of overt clerical-lay conflict is not prominent in either the Old or the New World.

In short, the book seems to promise more than it delivers. But this judgment should not be allowed to efface its value as an important summary of the persistent evangelical impulse in popular Presbyterianism on both sides of the Atlantic.

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THOMAS D. HAMM. *The Transformation of American Quakerism: Orthodox Friends, 1800–1907*. (Religion in North America.) Bloomington: Indiana University Press. 1988. Pp. xvii, 261. \$25.00.

Presenting a fresh area for studying American Quakers, this book, winner of the Brewer Prize of the American Society for Church History, will stand as one of the most important works in the field; it breaks new ground, shifting the study of American Quakerism from the eastern United States to the Midwest, modern Quakerism’s heartland. With compelling prose, Thomas D. Hamm describes the impact of modern ways on a religious society that in 1800 was insular, agricultural, and traditional. When his readers lay their copies aside, at the end of a too-brief 173 pages, they will have witnessed the transformation of an old order into a church that differs little from other Protestant denominations. If there are few bitter tears at the demise of what once challenged the broader culture, it is because most of the audience for Hamm’s work comprises jaded scholars who can name other examples of the same tale.

Based on broad research—the crammed endnotes reveal the archives, books, and articles Hamm has examined—the story weaves effortlessly through what was, by the mid-nineteenth century, a vortex of contending and fractious Quakers. Hamm explores the generational, economic, and social factors, as well as the theological

presuppositions, that led Friends to struggle for control of yearly meetings, whose participants were bled white by the conflicts. However difficult to improve on his overall interpretation, not all present-day Quakers, especially those who have honed their theological axes on parochial views of the past, will want to accept his findings.

Despite its value, this study has problems. The index is woefully and frustratingly inadequate, many actors simply unconsigned there. When they appear in the text, the reader longs for a bit more characterization and the author’s assessments. The pages are dotted with labels—“primitivist,” “tolerationist,” “liberal” (on p. 154, we even encounter two “liberal” factions within the same “modernist” grouping)—that remain ill defined and certainly do not prepare the reader for the nearly countless variety of Quaker factions, for example, Gurneyites, Hicksites, Wilburites, water-ites, and on and on. Historians, quite naturally, must impose some limit on what is a seamless past, but there should be a clear reason why the stitch is put here and not there. Hamm never discloses why he selected 1800 to start—he does mention that the Trent River Meeting in North Carolina sent an expedition to scout out Ohio, but he uses this detail to make a different point—and he claims that the reason for his concluding date, 1907, is self-evident, which it certainly is not. Why not end with 1902 when the Five Years Meeting (absent from the index) was created and begin with 1827, the year of the Hicksite separation?

Moreover, to illuminate a group that defined itself in reaction to others, Hamm’s attention span is too short and sometimes overly simplified. He skimps on information about the Hicksites and Wilburites, and once he perpetuates that hoary canard about the Hicksites being part of the “Unitarian-Universalist liberalism” of the 1820s (p. 172). In the same way, he focuses on Friends in the Midwest, in Indiana and Ohio, and manages to slight significant numbers of Orthodox adherents in North Carolina and the Northeast. And is it not helpful to limit evangelicalism, an admittedly difficult concept to define, to what he refers to as one central idea (p. 1), namely, an emotional conversion experience of the new birth. By that definition, a person with a profound mystical episode could qualify.

In sum, we have here an extremely useful volume, a path-breaking one whose interpretation future students will be obliged to deal with. Its less than fatal flaws should even serve to spark further refinement.

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PHILIP GLEASON. *Keeping the Faith: American Catholicism, Past and Present*. Notre Dame, Ind.: University of Notre Dame Press. 1987. Pp. vii, 285. \$24.95.

The small band of Roman Catholics that George Calvert brought to Maryland in 1633 could never have envisioned it. Outnumbered by Protestants in their own colony after just a few years, they were a beleaguered minority, surviving ultimately through a tenacious faith and an exquisite political tact. Not until the mass migration of the Famine Irish in the 1840s and 1850s did Roman Catholicism acquire a highly visible profile in the United States, but subsequent waves of Italian, East European, and Hispanic immigrants in the twentieth century made it by far the largest denomination in the country—sixty million strong in 1988. In this book, Philip Gleason takes an interpretive look at the American Catholic past.

Over the course of three centuries, Roman Catholics in America had to deal with a variety of contradictions and confusing challenges: how to maintain an authoritarian theological model in a democratic culture; how to assimilate dozens of ethnic groups without fracturing into dozens of separate churches; how to promote a corporate ideal in an individualistic ethos; and how to preserve religiosity in an increasingly secular culture. In this book, a compilation of articles, speeches, and lectures that he has developed in the last twenty years, Gleason reflectively looks at the major issues of American Catholic history, particularly in light of the controversies of the 1960s and 1970s, when American Catholicism went through a powerful identity crisis.

In "American Catholics and the Mythic Middle Ages" and "Catholicism and Cultural Change in the 1960s," Gleason examines the sudden rejection by American Catholics of the golden vision of the corporate, Middle Age ideology, as well as the romantic ideal, after Vatican II. The essays "Immigrant Past, Ethnic Present" and "Immigrant Assimilation and the Crisis of Americanization" look at the rejection of the assimilation model of American historians and the embracing of ethnic diversity as the most useful means of interpreting American Catholic culture. In "The Search for Unity and Its Sequel" and "Keeping the Faith in America," Gleason offers a personal reflection on the days before Vatican II, when "Catholicism was a comprehensive and integrated system of religious truth" and on the impact of Vatican II on people raised to accept that ideal world view. "Literary Landmarks of American Catholicism in Transition" looks at the intellectual impact of the demise of institutional Catholic associationalism in the 1960s when the election of John F. Kennedy

sparked the emergence of anti-ghettoism. Other essays deal with parochial school controversies, the impact of Catholic scholasticism, and the nature of Catholic historical consciousness. The book is carefully reasoned, clearly written, and uniquely insightful, what we would expect from one of America's premier religious historians.

JAMES S. OLSON

Sam Houston State University

JAY P. DOLAN, editor. *The American Catholic Parish: A History from 1850 to the Present*. Volume 1, *Northeast, Southeast, South Central*; volume 2, *Pacific States, Intermountain West, Midwest*. New York: Paulist. 1987. Pp. v, 359; iv, 423. \$24.95 each volume.

The parish, Jay P. Dolan notes in his introduction to these volumes, is "a key to understanding the history of American Catholicism" and "the most important institution in the [Catholic] community" (1: 2-3). Given its central position in the life of Catholics, it has received scant attention from historians. These volumes are an attempt to provide a social history of American Catholicism through a comparative regional approach that focuses on the parishes where average Catholics not only manifested their faith but often enough found a middle ground between immigrant and host cultures.

As an immigrant church, the Roman Catholic community has been overwhelmingly urban in the concentration of its members, never counting more than 15 percent of them from rural areas. Even in the South, most Catholics were clustered around the cities, although 70 percent of the parishes were rural, resulting in a long tradition of circuit-riding clergy that included "churches on wheels" by the First World War. Only in the West did Catholic parish life retain a missionary character until well into this century.

In the Northeast, the pastor became the dominant ecclesiastical figure in parish life by the Civil War with the demise of trusteeism. This particular parochial hegemony, Joseph J. Casino contends, was a necessary stage in the assimilation of the polyglot ethnic population into the larger society. And, more often than not, Casino finds, the effective pastor ruled by cooperation with the people rather than by bald authority. In the Southeast, Michael McNally finds that the lack of priests weakened their position and strengthened the family as the center of Catholicism. Significantly the trustee system remained a force in the South until 1900. That Irish clergy had by then begun to replace the French priests who had been dominant in much of the region may have had something to do with this change in polity. In the West,

the trustee system had little chance to develop. In the Midwest, "strong bishops," Stephen Shaw writes, "governed at times even stronger priests," while the laity was encouraged to organize itself for apologetic, spiritual, social, or other purposes, a development that enabled it to anticipate some of the reforms of the Second Vatican Council (2: 296).

In the Northeast, Irish domination led other ethnic groups to demand parishes of their own. Between 1880 and 1930, 30 percent of the new parishes were national; in cities like New York and Philadelphia they counted for more than half. Germans and Poles were particularly likely to take the initiative in organizing parishes, sometimes without episcopal authorization, and to claim the right to manage what they built. Thwarted claims of Poles in Buffalo, Chicago, and Scranton to control the appointment of priests led to schismatic movements in those cities.

In the South, ethnicity played a far smaller role in parish organization, with the exception of the black Catholic community, which had the burden of being a double minority. In dealing with blacks, the church followed the larger society, at first treating them as inferior members in common churches, then after *Plessy v. Ferguson* creating separate parishes for blacks, only to close them in the wake of *Brown v. Board of Education*. For the southern black Catholics who had often taken the initiative in forming their own parishes in the twentieth century, ecclesiastical integration was a mixed blessing. In the West, like the South, there was a reluctance to start national parishes, which would only reinforce the foreign image of the Catholic minority in these regions. In the Midwest the early heavy German presence made national parishes a necessity, with German Russians even receiving a diocese of their own in Bismarck, North Dakota, in 1910.

The parochial school, "the junior church" to George Cardinal Mundelein and many other Catholic leaders, became a dominant feature of Catholic America. The Northeast unsurprisingly led the way, with 60 percent of all parishes maintaining schools by 1960. Even there, however, they taught less than 38 percent of the Catholic students. In the Midwest the Germans were especially committed to Catholic schools, with 86 percent of their parishes having them. In the South and Far West, extremely limited resources, both material and human, enabled relatively few parishes to begin schools, but a number of Catholic public schools were set up in cooperation with local government authorities. Most failed to survive the renewal of nativism in the pre-World War I era.

By the 1950s the American Catholic parish was increasingly suburban, large, heterogeneous, and

impersonal. Ethnicity, neighborhood, anti-Catholicism no longer provided a natural bond for the parish. Then came the "Spiritual Earthquake" of the 1960s, whose shock waves the parish fully felt. The extent of the aftershocks is something about which the authors offer rather different judgments on their respective regions.

The volumes provide a useful historical perspective on the evolution of the Catholic parish in the several regions of the United States. Until much more local history is done at the parish level, however, the usefulness of such comprehensive studies will be limited. Only in a faint way can the reader of these volumes appreciate what Dolan cites as the aim of the study, to "begin to sense the pulse of the people and understand more accurately the texture and quality of their lives" (1: 3).

ROBERT EMMETT CURRAN
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MICHAEL J. McNALLY. *Catholicism in South Florida, 1868-1968*. Gainesville: University of Florida Press. 1982. Pp. xviii, 316.

This study of the institutional Roman Catholic Church, its leaders, and, to some extent, the laity begins with the creation of the diocese of St. Augustine in 1870 and concludes with the formation of the Archdiocese of Miami in 1968. Michael J. McNally attempts to explain how a missionary church of Irish-born priests at first fulfilled the needs of a diverse and poor Hispanic and Anglo population and eventually built a financially solid, multiethnic institutional apparatus worthy of recognition as an archdiocese. Perhaps because the author himself is a priest of the archdiocese, his analysis of those years also seems to point to how the future institutional church will implement Vatican II and make further efforts to unite the divergent and diverse people of that Christian community.

The author's major contribution to the history of American Roman Catholicism is his detailed account of the life of the priesthood and the growth of church governance. The various bishops are described as real estate agents, church and parish school builders, and founders of seminaries to train diocesan priests. Endless accounts of jurisdictional battles, personality clashes, and ethnic-racial differences among the priesthood capture the all too human qualities of the church's leadership. Despite early poverty and the paucity of Roman Catholics, the shrewd south Florida bishops built an institutional structure to handle the rapid changes that came to that region after World War II. The church leaders' heroic struggle against racism led to quality black parochial edu-

cation. The post-1958 multifaith Cuban settlement of south Florida is treated largely in terms of how bishops and priests responded institutionally and organizationally. The impact of changes in the liturgy on the priests and bishops is discussed as loss of institutional pride and changes in leadership activities.

As one gains knowledge of the role of church leaders as builders and sustainers of the Catholic community, that community, or its constituent population, does not receive proper attention. What south Florida blacks thought of the church and how they dealt with white lay Catholic oppression, especially during the New South period, go unstudied. The tremendous post-World War I migration to Florida of elderly, retired Catholics, while mentioned, is never related to how those old, northern, ethnic, city people adjusted to their southern and foreign colleagues and their southern church. Although McNally is sensitive to the differences in the Cuban Catholic faith, the social or religious needs of the Cuban laity are barely discussed. The author tantalizes the reader with mention of a laity disrupted by Vatican II's change in its eternal institutional faith, but he fails to develop either the implications of that assertion or how that change related to the tremendous post-World War II age, ethnic, and racial upheavals in the Catholic population.

Purportedly written to enlist the past to confront the changes among south Florida Catholics and their faith, this book ignores the questions of how to merge the study of the institutional church leadership with analysis of the laity. After all, the laity is the reason for the church's existence in the first place. Thus, dynamic stories of institutional growth overcoming diversity and the changing composition of the Catholic population are never merged. If the author wants to see how the past informs the future, he must look at the whole past.

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Catholic University of America

DONALD WEBER. *Rhetoric and History in Revolutionary New England*. New York: Oxford University Press. 1988. Pp. xii, 207. \$22.50.

During the last three decades, numerous scholars have explored the conjunction of religion and politics in the revolutionary sermons of New England ministers. In this sense, Donald Weber reenters what has become familiar territory for historians of early America. Once there, however, he employs such a wide array of innovative, interdisciplinary tools that yield such a host of imaginative insights that the whole landscape of revolutionary religious rhetoric takes on new hues and contours.

Every student of religion and the revolution must therefore reckon with the methods and message of Weber's provocative analysis.

Weber presents case studies of five patriot preachers—Philemon Robbins, Jonathan Edwards, Jr., Levi Hart, Stephen West, and Samuel Cooper—to depict the mental world of the evangelical clergy amid the upheaval of revolution (Cooper, an Old Light rationalist, provides counterpoint). Weber works hard to overturn the depiction of these New Divinity ministers as otherworldly and insists that they were profoundly attuned to communal anxieties over political events. As sensitive shepherds of their flocks, these pastors responded to their congregations' need for someone to make sense out of the cultural crisis of the 1770s or, in Weber's words, "to impose narrative order onto the experience of historical contingency" (p. 6); doing so gave them an oracular power and cultural authority unprecedented in colonial history.

But this achievement was neither easy nor lasting. Ministers, plagued with their own personal uncertainties (as revealed in private diaries and letters) about the course of secular history, could not sustain a narrative form in their sermons and opted instead for a style of pulpit discourse remarkably similar to the New Light preaching of the First Great Awakening. In the fragmentary syntax, vertical catalogues, and incantatory phrases of these revolutionary sermons, Weber finds a strong rhetorical link between revival and revolution. Once the revolution passed, so, too, did this style of preaching and the "secular engagement" (p. 149) of the New Divinity men. Thereafter, they recovered the narrative form of sermonic expression but retreated from worldly events into theological disputes, a change that by 1800 both contributed to and reflected their loss of cultural influence.

To reach these conclusions, Weber gives close textual analyses of extant manuscript sermons. His readings, aimed at recontextualizing the discourses of the revolutionary pulpit, are largely shaped by recent work in literary criticism, symbolic anthropology, narrative theory, and religious studies. Central to his argument are assumptions about revolutionary rhetoric as multivalent, the American revolution as a rite of passage, religious services as ritual process, and the breakdown of narrative in times of cultural crisis. Equally important is his conviction that the written forms of sermons can be as revealing as their intellectual content of an author's mentality.

Few readers will be comfortable with all of these methods and assumptions or the interpretations they lead to. Nevertheless, Weber's fresh approach to recovering the mentality of revolution-

ary ministers should serve to reinvigorate the religion and revolution debate, and, consequently, this study stands as a major addition to this literature.

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STEVEN ROSSWURM. *Arms, Country, and Class: The Philadelphia Militia and the "Lower Sort" during the American Revolution, 1775–1783*. (Class and Culture.) New Brunswick, N.J.: Rutgers University Press. 1987. Pp. xv, 373. \$40.00.

This is a challenging book, one that confronts the reader's abilities and sensibilities on several levels and that merits the diligence necessary to read and digest its contents. It begins forthrightly with pronouncements of philosophical and methodological perspectives and a clear statement of the author's agenda. "The philosophical stance that informs this work is . . . 'realism,'" Steven Rosswurm writes. "The theory that informs it is historical materialism or Marxism" (p. x). Rosswurm considers the writing of this book a "political act" (p. xi), and he chastises those historians who write "social history devoid of politics and the context of class relations" for producing "flawed history" (p. xii).

Having thus bravely laid his cards on the table, Rosswurm proceeds to play an extremely effective hand. He writes a fascinating history of the American revolution from the perspective of Philadelphia's "lower sort" within the overlapping contexts of power, class relations, and wartime service in the patriot militia. He posits a transformation in consciousness among the lower sort attributable in large part to the revolutionary experience, a short-term politicization of a previously excluded lower-class constituency, and an unsuccessful effort to realize a true social revolution within the confines of the political revolt against Great Britain.

None of this is, of course, surprising to those who follow the historical literature on urban radicalism in revolutionary America. Rosswurm's contributions come from the meticulous detail with which he charts evolving class relations of a particular time and place; his mastery of the complexities involved in such changes; and his sensitivity to the perspective of "lower sort" militiamen over the course of the war. He cajoles the limited sources, drawing from them every recoverable fact and any logical inference that might be drawn.

According to Rosswurm, the unique dynamic of Philadelphia's class relations contributed decisively to Pennsylvania's emergence as the most radical of the revolutionary states. The absence of an established militia system and the reluctance of

political elites to play a leading role at the beginning of the war help him explain the influence of lower-sort militiamen in setting the city's and the state's revolutionary course. "The militia transformed the laboring poor," Rosswurm writes; "the laboring poor transformed the militia" (p. 75). Both the poor and city politics were altered by wartime experiences.

Ultimately, Rosswurm provides a brief for the Philadelphia militia's oft-maligned behavior on and off the field of battle. He creates a logic for their actions consistent with their *mentalité* and the material conditions of their collective lives. They were not, he convincingly argues, consistently bad soldiers, but they did resent the disproportionate burden of the war that was carried on the backs of the lower sort, and their actions at particular times reflected both accumulating resentments and understandable concerns for the safety and subsistence of their families. The Fort Wilson riot was the decisive battle in a war for permanent alteration of sociopolitical relations among Philadelphia's classes, and, as is well known, the poor lost their revolution for social change.

One implication of the story, as Rosswurm tells it, is that a significant legacy of the war was greater conflict, the continued appearance of street demonstrations and violence; simultaneously a more intolerant and repressive attitude toward popular politics, mobs, and the poor in general; and an increased dedication by traditional political leaders to "order" in the city that became, briefly, the nation's capital. All of which provides missing details, long-term contexts, and continuities for the work of historians such as John K. Alexander, John R. Howe, Jr., and Jesse Lemisch; these details fit nicely within a historiographical paradigm populated by such scholars as Gary Nash, Sharon Salinger, Ronald Schultz, Billy Smith, and Alfred F. Young, among others. Rosswurm's contribution to this collective endeavor is a significant one; the book adds knowledge about one important part of our past that the guardians of this nation's historical myths prefer to deny or ignore.

THOMAS P. SLAUGHTER
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JOHN R. NELSON, JR. *Liberty and Property: Political Economy and Policymaking in the New Nation, 1789–1812*. (Johns Hopkins University Studies in Historical and Political Science, 105th series, number 2.) Baltimore: Johns Hopkins University Press. 1987. Pp. xv, 221. \$25.00.

John R. Nelson, Jr., sets an ambitious goal for himself in this book: a survey of national policy making in the United States between 1789 and

1812. He does not claim to have exhaustively examined the sources in search of the nuances of public policy but instead hopes to rescue the "results of new historical research" either from isolated and unrelated obscurity or from the "procrustean interpretive beds" into which such studies have fallen (p. xiii).

Nelson's theme is succinctly stated on page 170: "Despite its ultimate outcome, the political economy of the triumvirate [Thomas Jefferson, James Madison, and Albert Gallatin] stands as a more comprehensive and effective expression of liberal economic nationalism than [Alexander] Hamilton's vigorous but flawed efforts to procure a strong union through highly centralized government tied to Britain." Throughout the book Nelson attacks Hamilton's policies as "often corrupt and coercive," while venerating the attempts of the Republican leadership to formulate a public policy of vision tempered by political reality. Nelson succeeds in dislodging Hamilton's *Report on Manufacturing* from its often-proclaimed position as a fundamental document of national economic policy, an argument he first broached in the *Journal of American History* (1979). Instead, Nelson touts the collected writings of the triumvirate, especially Gallatin's *Report on Roads and Canals*, as the true beginnings of a national economic policy.

Nelson's book is provocative. He deserves praise for reminding historians that public papers such as Hamilton's *Report* deserve scrutiny with regard to their timing, political context, and content, that is, for what they do not say as much as for what they seem to say in the light of much later developments. Unfortunately, however, Nelson does not follow his own good advice. In venerating Gallatin, he overlooks the rosy prospects of a large surplus that made Gallatin's proposals plausible.

Whether the substitution of one idol for another is an advancement in the historiography of the period is debatable, but my greater concern is the way in which Nelson uses the term "political economy" to interpret "the world view of the principal policy makers" (p. xiii). Nelson implies that his understanding of political economy coincides with definitions familiar to them. He then asserts that to "analyze the politics and policies of this era as expressions of conflicting political economies is less an interpretive imposition than a systematic inference based on the world view of the principal policy makers" (p. xiii). This would be well and good if indeed the definitions were applied objectively and accurately reflected contemporary usage. Unfortunately, however, in his eagerness to dethrone Hamilton, Nelson fails to explore adequately Hamilton's own definition of political economy, which was, so Hamilton said in *Federalist*, No. 35, the ability to exercise power judiciously

in light of what the public will tolerate, especially with regard to taxes.

Apart from the fact that this is the only reference in the *Federalist Papers* to political economy, Hamilton's perception of what was viable as public policy in light of political reality makes his achievements in foreign and domestic affairs all the more remarkable and considerably less sinister than Nelson would have one believe. For his day and for his time as a public policy maker, Hamilton well understood the practical limits of what government could and could not do. He was no different from Gallatin in his willingness to explore the bounds beyond which he knew he could not go. The only difference was that Gallatin had an apparent surplus to work with and Hamilton did not.

Such simplifications of historical interpretation may appear unwarranted in a review, but the issue is not so much the role of political leaders in formulating public policy as it is how carefully the historian places those leaders and their ideas into the context of their time and into the process of change over time. From the outset, Nelson fails to explore in any depth the actual perceptions and uses of political economy by the leaders he studies. Unlike John Stuart Mill who, as one bad poet put it, "By a might effort of will / Overcame his natural bonhomie" and wrote *Principles of Political Economy* (1848), Nelson is limited by both his own biases and his insufficient attention to the words of those he chose to study.

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JUDITH A. MCGAW. *Most Wonderful Machine: Mechanization and Social Change in Berkshire Paper Making, 1801-1885*. Princeton: Princeton University Press. 1987. Pp. xv, 439. \$40.00.

Rejecting technological determinism, Judith A. McGaw argues that the mechanization of the paper industry was based on social choice, that "human social and economic decisions [were] accountable for the diffusion of machines and for most of the consequences of machine production" (p. 7). The author begins her study, which is organized into three parts, with the introduction of paper making into Berkshire County at the turn of the nineteenth century and then explores the technological, economic, and social changes that transformed the industry and the communities in which paper mills were located. Although McGaw gives considerable attention to Zenas Crane, she does not concentrate solely on a single person or firm. Her geographical center is Berkshire County, and she examines a large number of

different firms that together manufactured a variety of paper products. And this approach is one of the many strengths of the book. McGaw discusses superbly the many technological developments that occurred within the industry and the various types of machines available to manufacturers, and she assesses the reasons why manufacturers chose or rejected certain machines and processes. Her discussion of the sometimes complicated machinery is clear, precise, and comprehensible. Although her central theme rejecting technological determinism is a convincing one, and her analysis of labor's response to mechanization thoughtful, there still are several points that could be clarified or expanded.

McGaw's discussion of labor and of the early industrial transformation of many of these Berkshire communities is fuzzy. At several points she briefly mentions factory farms but does not explain fully why they are necessary for mill owners, who worked on them, and what the relationship was between the farm hands and the factory workers. The section on the effects of the Second Great Awakening on changing values appears more an appendage to rather than an integral aspect of the book. And the author's discussion of housework and its implication for factory employment is not convincing. Finally, the cultural values of the changing work force are not handled well. During periods of intense mechanization, manufacturers hired both Irish and French Canadian workers, yet the author does not explain how those groups differed in their responses to the introduction of new machines, the employment of married women, and factory discipline. Were their attitudes and values similar to those of traditional native-born workers? Many of these issues and questions parallel those found in early family-style textile mills. Were they solved in a similar fashion? Did textile and paper mill owners borrow from one another? This book hints that they did. McGaw has expanded the discussion of early industrialization and has raised provocative questions.

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BRIAN C. MITCHELL. *The Paddy Camps: The Irish of Lowell, 1821-61*. Urbana: University of Illinois Press. 1988. Pp. xiii, 247. \$24.95.

This is a disappointing book. The study of a single Massachusetts community with the largest Irish population outside Boston for a period of a single generation should have permitted a careful, detailed, and rich historical account. Historians familiar with the excellent studies of the women of

the Lowell mills and of America's showcase industrial city before 1850 would be eager to know how Lowell changed under the impact of massive Irish migration after the Famine.

Brian C. Mitchell's account is a sketchy, uneven story with serious deficiencies in both composition and substance. The research is inadequate. The analysis is weak. The writing is awkward. Maps and street diagrams fail to illustrate and clarify the Irish presence. The map of Lowell on page 43 is mostly unreadable, leaving the reader unable to use the street diagrams to locate the Irish neighborhoods within the city. Footnotes are misplaced and cite sources that do not provide evidence for the assertions made (p. 22, n. 37). Paragraphs begin with a discussion of one topic and end with an unrelated topic (p. 21). Rules of good scholarship are ignored. Figures on population are taken from secondary sources rather than from federal and state censuses. Controversial and self-serving quotations are presented as fact without corroboration. Mitchell could have made better use of the Lowell newspapers. He leaves largely unexplored the social life of the Irish immigrant community. He scarcely discusses the participation of the Irish in politics and their growing political power, both of which are central in most accounts of emerging ethnic conflict in Massachusetts history. In general, limited information is poorly presented.

The Irish connection to the Yankee upper class is often cited but never explained. Yet the factor of social class was crucial in the evolving relations between the Yankees, both upper and working class, and the Irish, particularly in the political upheaval of the 1850s that witnessed the overthrow of the Whig elite. The Irish alliance with the rich was critical in inciting nativist hostility toward the immigrants.

But the author is so offended by the nativist American party (which even respectable historians describe with the pejorative Know Nothings—without quotation marks) that he makes little effort to probe for understanding. In place of analysis he simply reaffirms the portrait of the nativists as irrational bigots and, like nearly all critics of the American party, dwells on the bizarre. The Massachusetts legislature in February 1855 created a joint special committee on the inspection of nunneries and convents. Mitchell borrows the label, the Smelling Committee, meaning a nosy, obtrusive body. Such an attitude is unlikely to expand our knowledge.

The book does have value. There is excellent information on changing Irish occupations and family employment patterns that Mitchell carefully compiles from census and mill records. There is valuable material on the Irish priests and their parishes, their rivalries and conflicts, and the

importance of the parish in Irish life. But we do not find a rich social history of the Irish immigrants and their adversaries. We have glimpses and scattered comments but no detailed accounts.

PAUL G. FALER
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Boston

PETER BENES *et al.*, editors. *Itinerancy in New England and New York*. (Annual Proceedings of the Dublin Seminar for New England Folklife, number 9.) Boston: Boston University. 1986. Pp. 256. \$14.00.

This uneven but often-stimulating collection examines itinerant arts and professions in the American Northeast before 1850. Based on the 1984 Dublin Seminar for New England Folklife, its subjects include traveling preachers, singing masters, dancing masters, lecturers, peddlers, entertainers, circuses, menageries, and portraitists. The overall scope of social change—from the world of John Winthrop to the world of P. T. Barnum—makes the achievement of thematic unity and coherence impossible, a problem compounded by the sometimes widely divergent concerns displayed by social historians and museum curators.

The one abiding preoccupation of all of the essays is the itinerants' role in democratizing and popularizing knowledge and culture. Itinerant ministers, both before and after the Great Awakening, helped break down church discipline among New England congregationalists; itinerant singing and dancing masters brought scored music, minuets, cotillions, and contra dances to rural communities; itinerant profile takers allowed ordinary folk to have their "likenesses" taken and introduced such technological innovations as the daguerreotype and physiognotrace; book peddlers helped circulate the latest prints on politics and the arts, some of which were subversive of traditional morality; circuses provided the first truly popular entertainment in America and pioneered in the use of innovative investment and merger schemes; musical clockworks, automata, dioramas, and panoramas (not to mention "sageant pigs" and "philosophical fish") offered traveling amusement on a smaller and more varied scale; perhaps most important, itinerant lecturers of various degrees of fame and charlatantry worked the lyceum circuit and—like virtually all of the other itinerants—honed their skills in advertising and self-promotion. No doubt it was partly as a result of the itinerants' efforts that the Scot David Wilkie found himself moved to say in 1837, "No nation in the world understands the science of

puffing more profoundly than the American" (p. 243).

The collection is divided into four sections, "Music and Religion," "Education and Rural Literacy," "Social Arts and Entertainments," and "Portraits, Profiles, and Daguerreotypes." The first three will be the most useful to social historians; the fourth will find its natural constituency among museum curators, art historians, and folklorists.

Overall, the most suggestive essays are the ones by Barbara Ritter Dailey on itinerant preachers in seventeenth-century New England. Donald M. Scott on the popular lecture in antebellum New England, and Peter Benes on itinerant entertainers in eighteenth- and early nineteenth-century New England and New York. Dailey offers a fascinating account of the subculture of dissent in early Massachusetts. Rightly emphasizing that the origins of New England dissent "are not to be found in 'declension' but in the radical propensity of the early Puritan movement" (pp. 38–39), she traces the familial, professional, and affective networks employed by Quakers, Baptists, and familists in spreading their sectarian messages. Dividing families as well as communities, the dissenters helped bring religious pluralism to New England well before the end of the seventeenth century.

Scott, in his essay, calls attention to the sharp break between colonial and antebellum forms of itinerancy, alluding to the "staggering number and variety of entertainers and performers, purveyors of this or that kind of lore or doctrine," who were coursing their way through New England and New York during the 1830s. He characterizes native-born white Americans who came of age after 1815 as a "dislocated generation," forced to make their way by their wits in an increasingly mobile and disjointed cultural world. The "useful" knowledge purveyed by Ralph Waldo Emerson, Wendell Phillips, and a host of lesser lights was, says Scott, the "cognitive equivalent of the frontier" (p. 69).

The same sharp break between colonial and postrevolutionary cultural forms is found in Benes's fine essay on the jugglers, rope-flyers, puppeteers, and animal handlers circulating through the late eighteenth- and early nineteenth-century Northeast. With assemblies in New England as late as the 1770s adopting laws against stage plays, vagrancy, and traveling entertainers, the ancient injunctions against using one's time "unprofitably" were still very much alive in the late colonial period. But, in the more culturally complex postrevolutionary world, itinerant entertainment became both legitimate and commonplace. Indeed, the flowering of popular culture in the late federal period is one of the major subthemes running

through most of the essays in this collection. When, in the single year of 1798, a "Pig of Knowledge" was exhibited in Newburyport and a "Sapient Dog" was placed on display in Salem, one might well conclude that the dead hand of Puritanism had at last loosed its grip on the culture of New England.

STEPHEN INNES
University of Virginia

VERA BRODSKY LAWRENCE. *Strong on Music: The New York Music Scene in the Days of George Templeton Strong, 1836–1875*. Volume 1, *Resonances, 1836–1850*. New York: Oxford University Press. 1988. Pp. lvi, 686. \$85.00.

George Templeton Strong, a cultivated, brilliantly articulate New York lawyer, kept his private journal over a forty-year period, from 1835 (when he was a fifteen-year-old sophomore at Columbia College) until shortly before his death in 1875. Initially published in 1952, Strong's account is filled with uninhibited observations on the social, cultural, and political history of his time. A life-long devotee of music, Strong heard bands playing at Castle Garden, regularly attended church concerts, listened approvingly to Henry Russell's melodramatic ballads at Niblo's Garden, and savored oratorios presented by the Sacred Music Society but until 1845 steadfastly avoided the theater, including operatic performances, for religious reasons. He heard Handel's *Messiah* for the first time in 1840, Beethoven's Fifth Symphony a year later, attended the inaugural concert of the New-York Philharmonic Society in 1842, heard violinists Ole Bull and Henri Vieuxtemps, and witnessed the Astor Place riot of 1849. He attended a concert version of Rossini's *Semiramide* at Castle Garden, Handel's *Samson* given by the Sacred Music Society, and finally, at age twenty-five, with much conscience-salving, heard his first staged opera, Weber's *Der Freischütz*, presented by the German Opera Company. In 1848 he married an opera lover and soon enthusiastically subscribed to the Astor Place Opera.

A musician herself and, as a scholar, largely responsible for the Scott Joplin revival in the 1970s, Vera Brodsky Lawrence began this study in 1976, intending to retrieve and publish with editorial exposition the entirety of Strong's comments on music. After painstaking research, Lawrence decided to round out the picture, using Strong's journal as a point of departure and adding her own account of the New York musical scene outside the journalist's experience. The result is a fully documented chronicle, assuming a binary form—complementary chapters cover a given

year. One set of chapters is devoted to Strong's entries, followed by a second set narrating musical events not recorded by Strong, drawn mainly from the contemporary press. Envisioned as a three-volume project, this initial installment of *Strong on Music* covers the years up to 1850 and leaves few notes unheeded. Lawrence writes with authority—sometimes engagingly, often to the point of tedium—yet the musical world of Strong evolves with pristine clarity.

Strong emerges as a fascinating personality—capable of social snobbery, intellectual intolerance, religious and ethnic bigotry. A bit of a melancholy recluse in his youth, he wrote with grace and wit, flavoring his commentary with opinionated pronouncements and discursive soliloquy. Often his first reaction to a piece tended to be negative, although this impression usually improved on second hearing. When Strong was introduced to the *Messiah*, he insisted: "There's too much science in it for my comprehension and quite too much fugue to suit my taste as yet" (p. 88). Beethoven's Fifth Symphony he found "generally unintelligible" (p. 111), whereas Mozart was "too artistical, it appears to me—too Addisonian and elegant for my taste" (p. 199). Gradually his critical faculties grew, and by 1846 he judged Beethoven's Pastoral Symphony "glorious, glorious, glorious" (p. 365). Slow to forsake his early predilection for sacred music, Strong tended to dismiss the operas of Donizetti and curiously found Bellini's *La Sonnambula* "worth three of *Norma*" (p. 444), widely acknowledged that composer's masterpiece.

Nineteenth-century New Yorkers were exposed to an assortment of music in concert rooms, theaters, churches, beer halls, cafes, in the streets, even on steamboat excursions. Lawrence includes it all, along with the musical criticism of Henry C. Watson, Richard Grant White, and such illustrious literary figures as Margaret Fuller and Charles A. Dana. This adds up to a rich tapestry, with Strong himself merely its central focus. Generously illustrated, Lawrence's initial volume is a model of scholarship—occasionally delightful to read, always a treasure trove for researchers, and usefully demonstrating, among other trends, the slim regard shown American composition during the mid-nineteenth century. Although the casual reader may become fatigued by the amount of detail, the often contradictory opinions expressed by Strong and his contemporaries on musical activities ranging from Swiss bell ringers and temperance concerts to grand opera and major symphonic premieres will prove invaluable to serious students of American culture. Lawrence's assemblage is exhaustive, her accomplishment a monu-

ment of historical compilation on a most professional level.

RONALD L. DAVIS
Southern Methodist University

MECHAL SOBEL. *The World They Made Together: Black and White Values in Eighteenth-Century Virginia*. Princeton: Princeton University Press. 1987. Pp. xii, 364. \$25.00.

This is a rich and insightful book designed to alter profoundly the way southern, and so American, history is understood. It is the thesis of this book—and it is very much a book with a thesis—that the great majority of those who settled Virginia (and by extension the South) brought with them a preindustrial world view and matching practices. Whether they came, in the first instance, from England or from Africa, they had a sense of time and work, place and space—a relationship to life, and death, and causation—that grew out of and was sustained by the natural rhythms of day and night and of the seasons. Mechal Sobel argues persuasively that it is not sufficient to stress, as recent scholarship has done, the comparative autonomy of black culture in the South; the emergent forms of white culture were profoundly shaped by constant close association with the bearers of African cultural traditions.

Throughout the colonial period in which the genesis of southern cultures took place, white bond-servants and artisans, bearing an English version of such a preindustrial culture, lived and most certainly worked side by side with slaves who bore an African version of it. One result was the slow-paced way of being that the increasingly clock-ruled North later designated “the lazy South.” The popular southern culture formed from the fusion of traditional African and persistent pre-Reformation English ways was so powerful and pervasive that it steadily undermined the attempts of eighteenth-century rationalizers—of whom Thomas Jefferson and George Washington were only the most conspicuous—and ensured that their own offspring would fail to internalize rigorous self-discipline of time management and would grow up “southerners” like the common folk, black and white, around them.

It is impossible in a short review even to begin to do justice to a book that has undertaken and accomplished so much, assembling a treasury of documentation (visual images as well as writing) in the process. I can only assert that reading this book is a must for all historians concerned with the multicultural genesis of the United States and then indicate some of the facets of southern distinctiveness that it illuminates.

From season-based time and work rhythms, the book moves to studies in popular African and English perceptions of nature, spirit presence, and causality. These themes are brought to focus on witchcraft—a manifestation evidently much more important in Virginia and southern history than has hitherto been allowed. Attitudes to place are next reviewed, focusing in a most evocative way on the cultural “imaginings” that invested this region of the New World. The contrasts between an Edenic vision and an instrumental, commodity approach to land are traced. There follows a survey of social space in the little house and the great house, with a review of the physical congestion of the small dwellings in which almost everyone—gentry and slaves alike—lived throughout the eighteenth century. In the course of this, there is a perhaps overextended look at the better-documented intimacies between whites and blacks in the Georgian and Palladian mansions into which a very visible superprivileged few were moving. The important recent work (by Henry Glassie, Cary Carson, Dell Upton, and others) on modes of house construction is linked to a literature (and photographic record) of African forms in a way that makes a very persuasive case for strong influences on the most important of all artifacts—the house and the home space—as they developed from the late seventeenth century onward in Virginia. This contribution to family history is followed by a further contribution to an already quite extensive literature on the naming of persons across generations and cultural traditions.

The third and crucial part of the book deals with “understanding of causality and purpose.” The author reviews the African and English senses of the origins of life and death, then turns to consider the waves of evangelical awakening that broke over Virginia society in the course of the eighteenth century, creating those networks of interracial sacred “families” that were the Baptist and Methodist churches and “classes.” In what may be the most important interpretive move relating to her thesis, Sobel assembles a mass of evidence to suggest that this surge of ecstatic religion has above all to be understood as a convergence, almost as a crossing over of Anglo and Afro religious trajectories. The blacks accepted an eschatology of fall, sin, and redemption and came to posit an ultimate causation arising not from personal motivations in the human and spirit world but from the single divine creator-redeemer; the whites entered into a trance-and-vision religion that had experiential validation from their intimately shared assemblies with black folk and came to share a notably African sense of immediate spirit-journey and of an ultimate going to be “home” among the ancestors.

A book with such a great, ingeniously and tenaciously pursued thesis must be expected to rouse controversies rather than to settle them. So much the better. Even a short listing of the questions that this book challenges the critical reader to raise would extend this review far beyond its allocated limit. No one now is likely to refuse to accept the broad thesis advanced by Sobel in its most general form—that African ways influenced emergent southern culture during the eighteenth century; her great contribution has been to address the subject and to launch a particularized discussion of such cultural transference. Her impressive scholarship thus opens a fertile field of research, model construction, and refinement of formulation, in which historians, and others concerned with cultural studies, must strive for better understandings of the extent of African influence and of the mixed systems of cultures that were coming into being in the South. They must explore the divergences (admitted by Sobel, p. 212 and *passim*) as well as the convergences and seek to interpret the total configurations that two cultural traditions made at different stages of southern and, beyond that, American history.

RHYS ISAAC
La Trobe University

LARRY E. TISE. *Proslavery: A History of the Defense of Slavery in America, 1701–1840*. Athens: University of Georgia Press. 1987. Pp. xix, 501. \$40.00.

This is an interesting book with a bold and startling thesis. In this history of the defense of slavery in the United States between 1701 and 1840, Larry E. Tise pointedly rejects the central arguments of all previous scholarship on proslavery thought. Proslavery was not, he argues, an aberration in American thought. Rather, it was “one of the clearest possible indications of the nature and character of American society and values throughout the nation” (p. xiv). Most important, Tise contends, it was not a body of thought that was uniquely southern. All of the basic proslavery ideas had been developed outside the South. They were given full expression in Great Britain, the West Indies, and in New England during the eighteenth century. They achieved their greatest importance and elaboration in America in the early nineteenth-century works of conservative New England Federalists of Congregational background. Hence, this history of the proslavery argument is filled with only a handful of scattered references to men traditionally regarded as proslavery thinkers. There is very little about George Fitzhugh, James Henry Hammond, or Henry Hughes, but there is a great deal of material on

Timothy Dwight, the Federalist president of Yale, as well as on dozens of conservative New England clergy.

Tise offers some of his most startling revisionist conclusions in his analysis of the proslavery argument during the period 1820–40. Contrary to the claims of previous historians, Tise argues that southerners did not make any significant contribution to the proslavery argument during the 1820s. Even the nullification movement of the early 1830s was not connected to the rise of southern proslavery thought. While southern thinkers sat quietly, northern antiabolitionists, drawing on the earlier thought of conservative New England Federalists, developed an elaborate proslavery ideology that swept the nation. Only belatedly, after the abolitionist postal crisis of 1835, did southerners add their voices to the discussion—but only to mimic ideas already developed by New Englanders.

One cannot help but admire Tise for his willingness to disagree completely with all previous work produced by historians of proslavery thought. Definitions and categories that remain unchallenged for very long have a way of restricting our imagination. Tise challenges everything that has long been held sacred by historians of the proslavery movement. Moreover, he offers us not simply a revisionist but a revolutionary thesis. He has severed proslavery from slavery and found its home in the very place where others had detected the origins of abolitionism.

Most significantly, Tise has redefined proslavery thought. His definition includes nearly all American conservative thinkers: those who favored an orderly society, those who saw virtue in obedience to “superiors,” and those who opposed abolitionism. Since he has redefined the concept to include all expressions of conservative thought, it should come as no surprise that he finds proslavery thought everywhere. His redefinition of proslavery determines his conclusions. Of course, in developing a definition that shapes his analysis, he is no different from all of the historians whose arguments he rejects.

Tise’s work, however, is significantly weakened by his insensitivity to context. He does not appreciate that the same words and ideas might carry different meanings in different contexts. A New England clergyman of the early nineteenth century and a South Carolinian of the 1830s might very well use the same language and yet mean quite different things. When New England antiabolitionists defended social order, they usually did not have in mind images of masters and slaves on plantations. But that was precisely the context understood by proslavery southerners and their audiences. Ultimately, by ignoring the larger so-

cial world that surrounds words and infuses them with meaning, Tise has magnified the significance of the connections between New England conservative and southern proslavery thought.

KENNETH S. GREENBERG
Suffolk University

RICHARD H. SEWELL. *A House Divided: Sectionalism and Civil War, 1848-1865*. (The American Moment.) Baltimore: Johns Hopkins University Press. 1988. Pp. xii, 223. Cloth \$29.50, paper \$9.95.

A narrative history of the coming and course of the Civil War, centered on the issue of slavery and suitable for assignment to undergraduates, is a worthy undertaking. Anyone who has recently taught a lecture class on the subject has probably wished for such a book to assign to students. As Stanley Kutler, the editor of the "American Moment" series (in which this is one of the first volumes), explains in his foreword, the Civil War represents both the tragic failure of "a system rendered morally deficient and politically paralyzed by slavery" and a rededication to the ideals of liberty and union (p. ix). Richard H. Sewell sets out to tell the story of the sectional conflict in terms of the central moral and political issue of slavery, without footnotes and in a way readily accessible to undergraduate readers.

Sewell describes his book as "a modest challenge to those historians who see slavery as a largely artificial or symbolic issue and who emphasize instead the role of such 'ethnocultural' concerns as temperance and nativism in shaping public events at mid-century" (p. xi). Sewell never really engages, however, with the ethnocultural school or incorporates its contributions into his own interpretation. Nor, for that matter, does his account show any recognition of other recent scholarly approaches such as modernization theory, neo-Marxism, the "new" political history, and the classical republican paradigm. One does not look for overt historiographical excursions in a narrative, but some application of these ways of looking at the events would have been appropriate and beneficial.

At times Sewell's execution falls short of his goal. The debate over slavery, which should be the centerpiece of his book given its interpretive thrust, is surprisingly slighted. The "free labor" value system is never explained or contrasted with that of the plantation. The religious revivals that formed the background of the crusade against slavery are not described; instead, religion is mentioned as one example of sentimental relaxation, along with songs like "Home, Sweet Home" and

"The Old Oaken Bucket." On this basis, students would have a hard time understanding the zeal of John Brown or the emotional significance of his raid. Occasionally, the substance of Sewell's message is betrayed by the breezy style in which he relates it. The death of a president at what was already a time of grave national crisis in 1850 is passed off with a flippancy. Succinct judgments do not have to be dismissive, like the description of Harriet Beecher Stowe's *Uncle Tom's Cabin* as "sugar-coated antislavery" (p. 37).

Such a volume as this probably requires a little more than the space apparently allotted to Sewell. One suspects the publisher of having imposed a two-hundred-page limit on his text, because the coverage is sometimes so thin. Particularly in the first three chapters, too many events are noted with little or no explanation, such as "the virtual collapse" of the Whig party in 1852 (p. 39). Students reading this book would be left with many questions that could easily have been answered in the text. Why were the nativists called "Know-Nothings"? What is "riding circuit"? What is the origin of the title of this book? And surely the complete absence of the Lincoln-Douglas debates from an account that purports to accord a position of centrality to the slavery question is peculiar.

The strongest part of this book is the second half, devoted to the war itself. There Sewell hits his stride, and his style takes on an appropriate sense of drama. What Bertram Wyatt-Brown calls "the humanness of the past" is more satisfactorily recovered. The role of blacks in the war is especially well handled, and what Sewell has to say about northern immigrants is interesting. There is an excellent bibliographical essay at the end. Although it does not entirely fulfill its promise during its hurried survey of the 1850s, this book serves a useful purpose within a well-conceived series.

DANIEL WALKER HOWE
University of California,
Los Angeles

MARK W. SUMMERS. *The Plundering Generation: Corruption and the Crisis of the Union, 1849-1861*. New York: Oxford University Press. 1987. Pp. xv, 362. \$29.95.

In this volume Mark W. Summers turns from the malfeasance and misfeasance he explored so effectively in parts of his *Railroads, Reconstruction, and the Gospel of Prosperity* (1984) to corruption in the 1850s. He argues that, although some historians have blamed the Civil War on a "blundering generation," the war was "also made more likely by the 'plundering generation' that so many Ameri-

cans thought had betrayed the spirit of the Founders" (p. 304). Conceding that there were many more direct roads to the collapse of the Union than the corruption route, he nonetheless concludes that it "was a Union sickening unto death that perished on that April morning, and the sickness had been growing upon it for a dozen years" (p. 304). The author is aware of most of the challenges and objections that will come and deftly fends them off with carefully phrased caveats. The nature of the study, however, is necessarily such that an unwary reader will obtain a distorted and exaggerated image of the extent of documented corruption and a misplaced confidence that a perception of corruption affected people widely and intensely.

The book is very effectively written and provides arresting accounts of numerous forms of impropriety scattered over a large majority of the states as well as the District of Columbia. It is based on an impressive array of sources and relevant secondary accounts. The author's sophistication, perceptiveness, skillfulness in analysis, and freshness of perspective enliven every chapter. Many forms of corruption are lucidly delineated and strikingly illustrated, including the use and abuse of the political spoils system, the prostitution of the press to politics and worse, violence and fraud in elections, and malfeasance among officeholders of low and high rank. A six-chapter section entitled "Pathology—the Corruption of Government Favors" explores lobbying, bribery, favoritism, influence peddling, and "Frauds Astounding Even for New York" (p. 138). The presumably devastating effect of corruption on the political parties receives penetrating treatment. And the theme of collapsing public confidence in political institutions as a contributing cause for abandonment of hope for sectional compromise is illustrated with a brilliantly woven tapestry of apt quotations.

Placing this image in reasonable perspective requires recognition that 31 percent of the illustrations of corruption relate to the big-city states of New York, Pennsylvania, and Louisiana and another 40 percent to peripheral and substantially frontier areas (Maine, Michigan, Wisconsin, Minnesota, Iowa, Nebraska, Kansas, Arkansas, Texas, Florida, and California). The eighty-million-dollar federal budget, several times cited as an impressive source of corruption, was no more than 2 percent of the annual value of farm and factory production of the time. And no panoply of quotations selected from the almost boundless documentary record can more than barely suggest the extent and intensity of popular conviction regarding the significance of corruption. Even the persuasive recounting of spectacular examples of corruption frequently has to depend on biased and

inconclusive sources. The allegations, however believable, were not always validated by official investigating bodies, and investigators not reaching damning conclusions were often charged with a similarly unproved corrupt cover-up. Nevertheless, with this carefully qualified but dramatically presented theme, Summers has challenged every serious student of the coming of the Civil War to become sensitive to a different perspective in re-examining some parts of the historical record.

THOMAS B. ALEXANDER
University of Missouri,
Columbia

WILLIAM C. HARRIS. *William Woods Holden: Firebrand of North Carolina Politics*. (Southern Biography Series.) Baton Rouge: Louisiana State University Press. 1987. Pp. xiv, 332. \$35.00.

The saga of William Woods Holden's political career is a remarkable story. Although he was the dominant, albeit controversial, figure in mid-nineteenth-century North Carolina history, his career ended with impeachment and removal from office. In fact, his character was so maligned that no southern historian made a serious effort to ascertain his correct role as influential newspaper editor, leader of three successful political parties, reform advocate, provisional governor after the Civil War, and finally governor in his own right. Within the past five years, however, three major biographical studies have been published on Holden, which collectively should rectify his tarnished reputation.

Overcoming his illegitimate birth and lack of formal education, Holden achieved his first success as editor of the *North Carolina Standard* (Raleigh). Fearlessly he led the disorganized Democratic party organ into one of the South's commanding newspapers and the Democratic party into control of state politics. Although he championed the southern right of secession, he was pro-Union in the 1860 election and supported the Confederate cause only after Fort Sumter. Subsequently he broke with the Democratic party and served as the standard-bearer of the state's peace movement. President Andrew Johnson appointed him provisional governor in 1865, yet, in spite of having a fairly successful administration in restoring federal authority, Holden failed for the third time to achieve his lifelong ambition of being elected governor.

Convinced that state leaders would never accept racial equality or other Reconstruction requirements, Holden embraced Radical Republicanism in 1867 as the best means of leading the state back into the Union. Although this won for him the

governorship in 1868, he was never able to implement his long list of democratic reforms. In fact, his administration seemed to catalyze the atmosphere of acrimonious intrigue and fraud, culminating in the rise of the Ku Klux Klan. Holden, with the full endorsement of President Ulysses S. Grant, attempted to crush the Klan militarily; thus, the so-called Kirk-Holden War. His efforts were so unpopular that, when his opponents gained control of the general assembly in 1870, they removed him from office, the first state governor to be so treated.

Through Grant's influence, Holden was named political editor of the *Washington (D.C.) Chronicle*, but his interests were in state rather than in national affairs. Consequently, he returned to North Carolina in 1872 as Raleigh's postmaster and served until 1881. In later life he steered an independent political course, regaining much personal respect from leaders of all political persuasions.

In this well-written and highly readable portrayal of Holden, William C. Harris achieves a scholarly consensus of previous studies while providing a "balanced account of the life of this complicated man" (p. 6). The end result, however, is a sanitized study, with the author making the mistake of being a synthesizer rather than an innovator who analyzes and interprets Holden's contributions. This book closely parallels two earlier biographies, one by Edgar Estes Folk and Bynum Shaw, *W. W. Holden: A Political Biography* (1982), and my own *William Woods Holden: North Carolina's Political Enigma* (1985), although Harris seldom acknowledges his use of either work. Harris, however, advantageously uses several newer sources—Marc Kruman's *Parties and Politics in North Carolina, 1836–1865* for one—and explores other recent historical developments and concerns. But he fails to show how Holden's personal or social sensitivities influenced his position on various issues.

Harris attempts to give equal and balanced coverage to the many facets of Holden's lifework, but inequalities are apparent. Especially noteworthy are his tracing of Holden's rise to power in "The Pope of Hargett Street" and his account of Holden in "Confederate Dissent." Yet there is insufficient treatment of family relationships, administrative policies and politics, Ku Kluxism, and Holden's impeachment trial. Harris is correct, however, in not attempting to psychoanalyze Holden's every wart or flaw. The answers to all of his contradictions and inconsistencies are not to be found in the source material that Harris used very carefully. Finally, although he correctly calls Holden a "firebrand," Harris fails to evoke the boldness, courage, integrity, vitality, or even the

vibrance that Holden demonstrated throughout his career.

HORACE W. RAPER

EMERITUS

Tennessee Technological University

JERRY THOMPSON. *Henry Hopkins Sibley: Confederate General of the West*. Foreword by FRANK E. VANDIVER. Natchitoches, La.: Northwestern State University Press. 1987. Pp. xix, 399. \$25.00.

In this book, Jerry Thompson presents an interesting biography of one of the South's least-known generals and also sheds light, long overdue, on the Confederates' most serious attempt to seize control of the far Southwest. In his discussion of Henry Hopkins Sibley's pre-Civil War career, the author's work reads like a virtual who's who in Civil War history as he introduces many young officers with whom Sibley served prior to that terrible fratricidal struggle.

Thompson opens his work in the style of a Russian novel with a lengthy and laborious introduction of the Sibley family from its arrival in Massachusetts in 1629 to the rearing of young Henry Hopkins Sibley in Louisiana. As a youth, Sibley was greatly influenced by life on his grandfather's plantation. There he was introduced to the cult of honor and the spirit of independence that would permeate his life.

Following his graduation from West Point in 1838, Sibley was assigned to the cavalry and sent to Florida to participate in the Seminole War. On his return to New York he met Charlotte Kendall whom he married in 1840.

During the next eleven years, Sibley fathered three children (two of whom survived childhood), fought in the Mexican War, struggled with the Apaches, served as a recruiter, and became a heavy drinker. It is not clear as to when, and under what circumstances, Sibley began drinking, but apparently it was a growing problem for the young officer as he traversed much of the West in the decade preceding the Civil War.

This series of adventures took Sibley on a circuitous route from bleeding Kansas to the Mormon War in Utah and to chasing Navajos in the Southwest. While assigned to help quell the problems in Kansas, he narrowly missed an opportunity to change the course of history when he came within hours of capturing John Brown. It was also at this juncture that the army first took serious notice of the Sibley Tent, a highly functional, conical-shaped tent that Sibley adapted from the Indian tepee.

Although he opposed slavery, Sibley cast his lot with the Confederacy over the objections of his

wife. He was given command of the New Mexican campaign; his friendship with Jefferson Davis was a deciding factor. Davis, who had his own vision of manifest destiny, entertained hopes that a successful campaign in the Southwest would win that territory, California, and international recognition for the Confederacy. The disastrous campaign was plagued with logistical problems from the outset. The hostile country and Sibley's drinking and ineptness further contributed to its failure.

After the war, Sibley went to Egypt, where his dreams of fame and recognition again escaped him. In the closing years of his life he sought solace in family and religion.

Thompson is no apologist for Sibley and candidly states that a more favorable biography of the Confederate general might have been possible if Sibley's personal correspondence had been available. Throughout the work Sibley constantly appears as a pathetic figure whose military career was dogged by one court martial after another. Although he was never found guilty of the several charges brought against him, he nevertheless seemed to be miscast by fate and incapable of leading great campaigns and military conquests. The author sees Sibley's failures as the result of chronic illness and an addiction to alcohol.

JAMES W. DADDYSMAN
Alderson-Broaddus College

CARL A. BRASSEAU. *The Founding of New Acadia: The Beginnings of Acadian Life in Louisiana, 1765–1803*. Baton Rouge: Louisiana State University Press. 1987. Pp. xiv, 229. \$24.95.

Of the Acadians forcibly deported from Nova Scotia by the British between 1754 and 1764, about three thousand made their way to French, and then Spanish, Louisiana. Some went directly to the tiny port of New Orleans while others arrived after sojourns in the British Atlantic colonies, France, and the French West Indies. The largest group, numbering some sixteen hundred, came from France in 1785 to join about twenty-eight thousand Louisianans, over one-half of whom were slaves. The Acadian experience in Louisiana is traced clearly and in detail in this carefully researched study by the descendant of a Cajun exiled first to Maryland, who then journeyed to the gulf colony.

Carl A. Brasseaux's initial chapters present a portrait of Acadian life in maritime Canada from the original Huguenot commercial ventures in Nova Scotia through the dispersal. Two chapters then describe the miserable life of the unwanted and distrusted exiles in Maryland and Pennsylvania and in France. Chapters 5 and 6 bring the

Acadians to Louisiana and analyze their patterns of settlement in three separate areas, one along the Mississippi River, a second along the upper half of Bayou Lafourche, and a third in the prairie country of southwestern Louisiana. Five chapters follow that investigate the evolution of Acadian culture with particular attention directed to the anticlerical nature of Acadian Roman Catholicism, Acadian relations with the Creoles and with the Indians, and, finally, the impact of slavery on Acadian society.

Focusing exclusively on the Acadians, Brasseaux makes no effort to compare the frontier experiences of his people with other equally discrete groups of pioneers in North America. The absence of comparative analysis presents serious problems because the author wants the reader to recognize the Acadians as a unique society. Time after time, Brasseaux tells us that New World history crafted the Acadians into a society that was committed to the extended family, highly individualistic, culturally conservative, hostile to outside interference, and socially egalitarian. They may have exhibited all of these qualities, but that does not make them unique. A convenient group against which to measure the Acadians would have been the early German settlers on the German Coast and those who followed them, who had fashioned a small-farm agricultural system by the 1760s. To what degree were the Acadians better prepared than other homogeneous frontier groups to preserve the old ways and resist the new?

A good example of the disadvantages of ignoring the experiences of other groups becomes clear in the discussion of Acadian litigiousness over land claims. Within the Spanish colonial system, such aggressiveness regarding property rights was hardly unique. None appeared in the courts more often during the seventeenth and eighteenth centuries than the small band of New Mexicans in and around Santa Fe. Such behavior occurred when the population expanded to or beyond the ability of nearby land to sustain it. Furthermore, legal contests must have pitted Acadians against Acadians. Did this, perhaps, fray their sense of collectivity?

Brasseaux depicts the Acadians as a nonacquisitive and egalitarian society. By 1800, however, large numbers of Acadian families were slave owners. Brasseaux, in effect, blames this circumstance on the manipulative politics of the French, Spanish, and British, each fostering racist social relations with Indians and blacks. Acadians, it would seem, swiftly abandoned egalitarianism and nonmaterialism when the economic opportunity arose to purchase land and the slave labor required to make it productive. Acadians with the

necessary capital simply responded to market incentives. Once owners, they adopted the same harsh attitudes toward their slaves as other owners. These slave-owning Acadians embraced a set of new values to accommodate their changed status. Brasseaux insists that slave owning and wealth changed Acadians only superficially.

Brasseaux's work contains much that is interesting, indeed fascinating, about this small ethnic minority. They were obviously a strong and communitarian people, real survivors, dedicated to the unity of the group. For the most part, the author maintains a nicely balanced objectivity in his depiction of his ancestors. But, at key points, he substitutes justification for explanation, thereby avoiding the possibility of discovering a new breed of Acadians, perhaps less personally acceptable than the old.

JOHN G. CLARK
University of Kansas

ELLIOTT ASHKENAZI. *The Business of Jews in Louisiana, 1840-1875*. (Judaic Studies Series.) Tuscaloosa: University of Alabama Press. 1988. Pp. x, 219. \$22.95.

Despite all that has been done in nineteenth-century southern history during the past twenty years, important areas remain virtually untilled and await cultivation. Facing diminishing returns on their labor, many scholars stubbornly continue to be largely occupied and to a degree obsessed with studies in politics, race relations, and the Civil War, thus overlooking fertile topics in business and economic history as well as the history of science and technology—vistas of the past that hold enormous promise for enhancing our historical understanding. One such new departure is Elliott Ashkenazi's book, which focuses on Louisiana's Jewish business community and its practices. Although relatively small in number, Jewish business leaders had unique and significant roles and functions in Louisiana and elsewhere in the South and did much to sustain regional economic growth both before and after the Civil War.

Ashkenazi in part builds on the previous studies of Bertram W. Korn, especially Korn's *The Early Jews of New Orleans* (1969). Ashkenazi's work, however, is distinctive in that he draws on a considerable amount of previously unmined archival materials and emphasizes business transactions rather than the customary religious or institutional themes within the field of Judaic studies. Ashkenazi maintains that Louisiana's Jewish business community, characterized not only by its mobility but also by its frugality, followed distinctive practices and played a crucial part in the

southern economy by performing mercantile and capitalist functions shunned or avoided by the elite planter class. Indeed, Jews filled vital roles within the economic matrix of the day, whether it was in the peddling of goods in the countryside or in the operating of market stalls located in New Orleans.

Ashkenazi's story begins in 1830, a time when Louisiana's cotton- and sugar-based economies were booming and when the first wave of French- and German-speaking Jews began to arrive in the state. These immigrants came from small European towns and villages set in an agrarian economy and discovered a similar situation in Louisiana. Quick to perceive needs and to seize opportunities, these people were different from Jews living in towns like Charleston and Savannah; they were the product of a dynamic economic environment expanding in many directions. This external force did much to alter customs and culture. Thus, it was business associations and family connections rather than the synagogue, so significant in many other communities, that ultimately shaped the group's social cohesiveness.

By the 1850s the Jewish business community in Louisiana was firmly established, prosperous, and growing in numbers. As a result of the group's deeply rooted communal autonomy—the consequence of its historical experience—Jews living in Louisiana avoided political controversy as the nation drew closer to disunion and war. With the coming of the Civil War, Jewish business leaders responded in a myriad of ways to the new circumstances, including some who profited from illicit wartime trade. But what is most important to this story is that with the conclusion of the conflict they emerged in a strong position to invest in land, funnel outside funds into various enterprises, and assume an even more important role within Louisiana's Reconstruction economy.

Although Ashkenazi succeeds in several ways in depicting Louisiana's mid-nineteenth-century Jewish business community, his work has some serious shortcomings. Ashkenazi's effort to examine business transactions in microscopic detail results in a monotonous and ponderously written account containing little life when dealing with the story's key figures. Outside of ledger sheet descriptions, the reader has little knowledge of the hopes, dreams, aspirations, frustrations, and successes of the individuals concerned. And the author tends to repeat himself from chapter to chapter, making this work most difficult to read. Perhaps a stronger editorial hand should have been exercised on the manuscript.

Nevertheless, Ashkenazi's work is a significant contribution to scholarship in the areas of Judaic studies, immigration history, and the history of

the South. The author has tackled a long-neglected topic and has enhanced our perspectives of a complex, vibrant, and heterogeneous culture.

JOHN A. HEITMANN
University of Dayton

DARREL E. BIGHAM. *We Ask Only a Fair Trial: A History of the Black Community of Evansville, Indiana.* (Blacks in the Diaspora Series; Midwestern History and Culture Series.) Bloomington: Indiana University Press, in association with the University of Southern Indiana, Evansville. 1987. Pp. xiii, 286. \$20.00.

Darrel E. Bigham's choice of Evansville, Indiana, as the subject of this study doubtless was influenced more by propinquity than anything else, but this particular city does, nevertheless, possess some interesting attributes that make an examination of its black population worthwhile. It is a small city (the federal census marshals counted more than one hundred thousand inhabitants only once before 1950), separated from a loyal slave area only by the Ohio River and located in a free state that not only had a long history of antiblack sentiment but also was one of only four northern states in which Democrats had a good chance of electoral success between 1870 and 1900. There was essentially no black presence (fewer than one hundred black residents) in pre-Civil War Evansville, but that portion of the population grew rapidly in the postwar years, reaching about seventy-five hundred (almost 13 percent of the city's population) in 1900. Thereafter, the number of black inhabitants declined or remained practically stagnant until the 1940s. The post-Civil War immigration came mostly from Kentucky, with natives of that state constituting roughly 40 to 60 percent of the Evansville blacks (far outnumbering Indiana natives) throughout the last third of the nineteenth century.

Bigham divides his work into four sections. The first (pre-1865) and the last (1930-45) are prologue and epilogue—brief (but more illustrative than impressionistic) and broadly encompassing. The post-1930 chapter captures surprisingly well the rather dramatic changes of the World War II era that laid some of the groundwork for the post-1945 civil rights revolution. The real meat of Bigham's book, however, is in the two central sections, dealing with the periods 1865-1900 (five chapters) and 1900-30 (six chapters). In each of these sections he examines population characteristics, housing, segregation and restrictions, occupational and economic opportunities (or lack thereof), family structure, black organizations and societal structures, and politics and voting. The

picture that emerges is, in several respects, one of slight change prior to 1930. Evansville blacks experienced from the beginning of the post-Civil War era the segregation and discrimination denoted by the term "Jim Crow" and endured them (only slightly alleviated) almost to the end of the era. Although the details changed, the employment patterns and chances of significant economic success for all but a very few were, in 1930, very similar to the situation in 1890. Black residences continued to be packed into a restricted area in the second and seventh wards (and a few other sections), and the index of segregation rose (though that may well have been a product of the change in the size of the reporting units).

In two areas, however, some changes were apparent. As might be expected, black organizations became more numerous and societal structures more complex. These shifts, in turn, led to greater diversity—and competitiveness—of leadership. More surprising, a combination of circumstances produced a decided increase in black political power. The narrow balance of the parties in the white community and the concomitant importance of the black vote created a situation made even more volatile by the early emergence of a black Democratic element and the tendency of the Republicans to split into factions. These developments produced an unusual situation in which a large and crucial block of black votes went, in 1925, to a Klan-backed Republican mayoral candidate who, when elected, made more black patronage appointments than any of his predecessors.

Bigham has ranged widely and dug deeply for his data. His work rests most heavily on contemporary newspapers, city directories, city and state documents, and federal census data, but he has also used an enormous mass of other primary materials and secondary works. There are times when I would have welcomed a little more care in proofreading; there are occasional awkward sentences and a few instances when different figures are given at different points in the book for the same data. But these are minor matters. Bigham's work is diligently researched, sensitively understood, carefully analyzed, clearly organized, and competently presented. It adds significantly to our understanding of urban postbellum blacks, especially in the smaller cities that are so often ignored. It is worth the years of work that Bigham has invested in this task.

LEONARD P. CURRY
University of Louisville

PAULA PETRIK. *No Step Backward: Women and Family on the Rocky Mountain Mining Frontier, Helena, Mon-*

tana, 1865–1900. Helena: Montana Historical Society Press. 1987. Pp. xix, 206. \$19.95.

Although most chapters of this study have appeared as articles, Paula Petrik draws them together with a discussion-provoking thesis. Petrik argues that “the urban mining frontier transformed its inhabitants, particularly its women, and promoted increased economic opportunity and social equality for its oftentimes unwilling change-lings” (p. xiii). She adds that the long-term result was a new definition of womanhood and that divorce proceedings in particular demonstrated that women expanded their independence and control. According to Petrik, “something was at work in the lives of both western men and women that altered their perceptions of gender roles” (p. xv).

Petrik uses the mining town of Helena, Montana, during the closing decades of the nineteenth century to test her generalizations. She offers a fascinating description of Helena’s development after 1865 and then focuses on prostitution, women’s generational differences, divorce cases, and women’s suffrage. She provides lively description to which she appends carefully collected statistical data as additional evidence. Petrik weaves a careful, well-documented story of women’s lives in early Helena that leaves little doubt that some interesting and significant factors helped shape those lives.

But the uniqueness of the West in altering nineteenth-century gender expectations is not as clearly established. For example, although the western divorce rate was higher, ideas about men, women, and marriage were changing in other regions as well. Numerous easterners, dissatisfied with their marriages and evidencing similarly altered expectations as westerners, temporarily migrated to a western town with a short residency requirement that also offered rail transportation, lodging, and entertainment facilities. These migratory divorces inflated western divorce rates. And, although it was true that women obtained a majority of the divorces in the West, this fact does not indicate anything highly significant, for this circumstance was also true in all regions and eras, including colonial New England.

In addition, liberalization of ideas and laws concerning divorce happened in virtually all states during the late nineteenth century. Montana was not alone in adding the ground of mental cruelty, for many states, including several conservative southern states, adopted it or a similar provision. Moreover, several scholars have suggested that the general liberalization of divorce codes, especially those relating to women, were not a reflection of the changing status or independence of women

but were actually an attempt to protect the institution of patriarchal marriage by providing redress against its abuses.

These few examples of intervening factors suggest that it is not easy, as Frederick Jackson Turner’s disciples discovered, to prove the uniqueness of the West. Although intuitively I agree that the West did, and still does, have its own personality that affects its inhabitants, I find it an elusive thesis in terms of indisputable evidence. Petrik’s fine study gives us additional motivation and material in the long quest to determine the scope and effect of the influence of the American West.

GLEND A RILEY

University of Northern Iowa

ROBERT M. UTLEY. *High Noon in Lincoln: Violence on the Western Frontier*. Albuquerque: University of New Mexico Press. 1987. Pp. xiii, 265. \$22.50.

Some time ago, a researcher bounced into my office and announced in a high-pitched voice, “I am here to do a book on range wars—the Johnson County War is first.” I sighed and silently thought “not another one.” I confess that, when asked to review a book on the Lincoln County War, this twenty-year-old conversation floated back—“not again,” I thought. The thought was an injustice, for this book is a masterful account of a flickering moment in western violence that has become enshrined in legend, song, and television. So detailed has been the research and so graphic the narrative that Robert M. Utley’s book should be the final statement (solution?) on the Lincoln County War. Of course, it will not be; indeed, it is foolhardy even to think that insidious thought.

Up front, the author tackles the filmy question: why was the Lincoln County War significant? Utley responds succinctly with four points: the war was a display case for the behavior of frontier personality types; the war laid bare the economic contest that underlay many frontier conflicts; the Lincoln County War revealed, without gloss, a spectrum of frontier violence; and, finally, that the war gave us, in perpetuity, Billy the Kid, bigger than life, as big as folklore would create him.

Not surprisingly, Utley discovers no heroes in the world of Lincoln County, no black and white morality, only banal gray. Greed, lust, and self-righteousness were motives on both sides. That stiff-necked John Henry Tunstall and his crowd employed the identical tactics of intimidation, violence, and threats that their opponents did—Lawrence Murphy, James Dolan, James Riley, and later the Catron rings.

Reduced to a sentence, the war “was a struggle to determine whether an existing monopoly or an

aspiring monopoly would dominate Lincoln County" (p. 112). Although a peace pact was signed on February 18, 1879, a year to the day after the hostilities began, the region's economic diversification with the arrival of mining and livestock booms replaced the smoking armament of frontier violence.

In selecting a structure for the interpretation of Lincoln County vigilantism, Utley compares the land of Lincoln County with the incisive model proposed by Richard Maxwell Brown. He finds striking differences. Instead of a horizontal class framework of the "conservative" and "better" elements against the "common" mob, Utley notes that the social hierarchy in Lincoln County split vertically with representation of all social strata on both sides. Nor, interestingly enough, does Utley discern any ethnic conflict. The Lincoln County War was basically an Anglo theater—the ethnics wanted no part in the drama (though a few played a part).

I would demur to Utley's proscription of the code of the West as a dominant factor in frontier violence. His research would lead him to the conclusion that the code of the West produced more nonviolent conformity than subscription to violence.

Utley can be proud of the latest contribution to his long and distinguished line of Western historiography. How he enjoys his subjects and how clear and effective the style!

GENE M. GRESSLEY
University of Wyoming

DAVID MONTEJANO. *Anglos and Mexicans in the Making of Texas, 1836–1986*. Austin: University of Texas Press. 1987. Pp. xii, 383. Cloth \$29.95, paper \$12.95.

Chicano history during the last two decades has challenged earlier and more traditional views of southwestern and western history. Writing from white male-centered perspectives, historians have generally seen the conquest of the Mexican region by the United States in the nineteenth century in only positive terms. Colonial rule, genocide of American Indians, racism toward Mexicans and Asians, labor exploitation, and oppression of women have been given little attention in what David Montejano calls the "triumphalist literature" of the Southwest and West. Yet substantial contributions to Chicano and other ethnic historiography, as well as to the history of western women, have forced a revision—albeit still incomplete. The result is a more complex view of the trans-Mississippi West as a place of conflict as well as of progress and cooperation.

Montejano's book (winner of the 1988 Frederick Jackson Turner Prize) exemplifies this revisionist historiography and, indeed, advances it. As a historical sociologist influenced by the work of Immanuel Wallerstein on world systems and Barrington Moore, Jr., on peasant transformations, Montejano's objective is to understand, in terms of race and class, the evolution of the Mexican-American in Texas. Pleasantly devoid of cumbersome and abstruse theoretical jargon, Montejano's work is a narrative, not a treatise. Yet his theoretical position clearly emerges from his investigation of key secondary works as well as vital primary sources accumulated by economist Paul S. Taylor, whose studies of Mexican labor in South Texas in the 1920s and 1930s are landmarks of what later was called Chicano Studies.

Montejano's main thesis is that the particular class and racial system imposed on Mexicans following annexation to the United States arose out of socioeconomic changes in Texas and all along the U.S.-Mexican border. Montejano outlines four class-racial orders in four key periods: 1836–1900, incorporation of Mexicans and the undermining of the earlier Spanish-Mexican *hacienda* system; 1900–20, reconstruction, when the agricultural revolution overturned the Anglo-Mexican ranch society; 1920–40, segregation and labor repression arising from the new commercial agricultural system and its need of a guaranteed pool of cheap labor; and 1940–86, integration as a result of a new urban-industrial order that has afforded Mexicans with what Montejano terms more "assimilative opportunities." Using this form of organization, Montejano analyzes the evolution of class and race distinctions in Texas vis-à-vis Mexicans. Avoiding the cumbersome artificial paradigms of race and class, such as the internal colonial model of political scientist Mario Barrera (*Race and Class in the Southwest* [1979]), Montejano concludes correctly that race and class discrimination against Mexicans has been the result of the incorporation of Texas into a regional and national capitalist economy that is part of, to use Wallerstein's term, a world system. Rather than viewing Mexicans in terms of a marginalized colonial system, Montejano sees them as an integral part of the transformation of South Texas from a semicapitalist society in the nineteenth century to part of a U.S. and world capitalist power in the twentieth. Montejano's book is not a revelation of new facts but a fundamental revision of how we analyze the history of Texas and the Southwest—and of the role of Mexicans in this history.

Montejano's work, despite its importance, does have some problems and limitations. The title, for instance, suggests a study of the entire state, but the book deals with the southern region only.

More important, it is less a study of Anglos and Mexicans than an analysis of Anglo elites, their transformation of the socioeconomic order, and the resulting oppressive class-racial order. Third, because Mexicans are not considered as primary actors in Montejano's history, they unfortunately are portrayed as passive victims of Anglo elites. Only in the last portion on integration does Montejano recognize the struggle of Mexican-Americans to change the system and their status in it. Finally, this study of class and race is actually a more orthodox analysis of class in which race appears primarily as a "rationale," "explanation," or "justification"—to use Montejano's own terms—for the class exploitation of Mexican landowners first and wage-agricultural workers later. Montejano pays little attention to more complex views of race as a dynamic factor as important as class in explaining the position of racial minorities in the U.S. and other Western societies. In this sense, Montejano does not—despite his insistence—employ what he incorrectly terms a "relaxed class analysis."

Nonetheless, Montejano's book is a major contribution to the revisionist historiography of the Southwest and West. Linking race and class to capitalist development is not new in Chicano historiography, but Montejano gives the linkage primacy by his focus. Montejano's analysis of the changing class composition of Mexicans along with its racial aspects supersedes the earlier but clumsy internal colonial theory advanced by Barrera, Rodolfo Acuña, and several others.

MARIO T. GARCIA
University of California,
Santa Barbara

DARWIN H. STAPLETON, *The Transfer of Early Industrial Technologies to America*. (Memoirs of the American Philosophical Society, number 177.) Philadelphia: American Philosophical Society. 1987. Pp. x, 215. \$18.00.

Early in this compact volume, Darwin H. Stapleton rightly points out that industrialization "wherever it has occurred," including the first industrial revolution in England, has involved technology transfer (p. 1). Attempted transmission of a technology from one nation or culture to another by way of written word alone, or even by shipment of technical equipment, has historically been less successful than when technically skilled migrants have carried the technology in person. While special-purpose groups and cultural minorities have been successful carriers of technology, "individual transfers have been the most

common," but they have also been the "most likely to fail to transfer their skills if those skills were new and innovative" (p. 31). Likely or not, after this caveat the book focuses on the ultimately successful activities of certain individuals in transplanting specific European industrial technologies to American shores by the middle of the nineteenth century.

Thus, after a thoughtful introductory chapter, which does set a wider stage, Stapleton presents four case studies rather than a comprehensive discussion of early industrial technologies. He has left to others, for instance, the monumental subjects of technology transfer in textile manufacture, steam- and water-power generation and application, and mechanical engineering. He offers instead a chapter each on the transplantation of canal engineering by English sojourner William Weston and English immigrant Benjamin Henry Latrobe, railroad engineering by American-born and European-trained Moncure Robinson, gunpowder manufacturing by French émigré Eleuthère Irénée du Pont, and anthracite iron making by David Thomas, who resettled in America from Wales in 1839. Some of these men ranged more widely geographically during their careers, but their actions in these cases took place primarily in the mid-Atlantic region of eastern Pennsylvania and Delaware.

Although these are success stories, Stapleton conveys a sense of the organizational, technical, and economic uncertainties and difficulties encountered in adapting European-developed technologies to New World conditions. He plays down the importance of personal charisma in overcoming these difficulties, but it is clear also that the protagonists in these narrations were talented, hardworking, and persistent individuals, not merely passive carriers of cultural tradition.

Where the discussion touches on truly technical matters at all, it is pitched at an introductory level. Each chapter tells a separate story, instead of drawing comparisons between, say, the financial organization of du Pont's enterprise as a French *société* in 1801 and the initiative of the Lehigh Coal and Navigation Company one generation later in importing David Thomas from Wales. The introduction only implies that the company's action was characteristic of a more "mature" era (pp. 25, 29, 31). Laudably generous footnotes and a useful selected bibliography encourage further exploration of such topics. As befits the state of the art, this book is strong on scholarly, substantiated narrative detail and modestly short on theory.

CAROLYN C. COOPER
Yale University

EDWARD N. AKIN. *Flagler: Rockefeller Partner and Florida Baron*. Kent, Ohio: Kent State University Press. 1988. Pp. xiii, 305. \$24.00.

As the subtitle makes clear, this book is a biography of a business leader with two careers, one better known than the other. Henry M. Flagler (1830–1913) was a pioneer in the nineteenth-century petroleum industry and in twentieth-century Florida real estate and railroading. Flagler's years with John D. Rockefeller and his contribution to the rise of the Standard Oil empire have received more scholarly attention than his Florida projects. Even so, Edward N. Akin has something new to say on both. He revises some older views and adds new information on Flagler's central role in Standard Oil's early history, and he provides a succinct and informed account of the former oil man's leadership in transforming Florida's economy. Both stories rest on strong research, not only among standard secondary accounts but also in published and unpublished sources and other contemporary materials.

The Flagler that is portrayed in this fair and interestingly written biography is an energetic and shrewd businessman, alert to new opportunities and driven to realize them. The son of a poor Presbyterian minister, Flagler rose from a clerk in Ohio country stores to a successful grain merchant (the business that introduced him to Rockefeller), went bankrupt manufacturing salt in Michigan, returned to the grain trade in Ohio, and in 1867 joined Rockefeller and Andrews, the oil refining partnership that in 1870 was incorporated as the Standard Oil Company of Ohio. Akin's review of Flagler's pre-Rockefeller years provides interesting insights into the larger business problems of the 1850s and Civil War decade. The salt business taught Flagler the advantages and disadvantages of competition and combination. He applied what he learned from that costly experience to controlling competition in the oil refining business, using the rate wars of the eastern trunk lines to assist Standard Oil in becoming the nation's dominant refiner. It was not only his success as Standard's chief "rate negotiator" that earned Flagler a top place in the company's management but also his role as a close Rockefeller adviser. He soon became the company's principal legal talent who, together with the corporation lawyer Samuel C. T. Dodd, drafted the 1882 trust agreement that allowed the Standard Oil properties in different states to operate as a single enterprise. Once the trust was organized, Flagler's direct involvement in Standard's operations lessened markedly, and, although he continued to serve the oil giant in important ways for another quarter-century, his

major business interests became increasingly fixed in Florida.

Flagler's Florida business dated back to 1885, two years after an initial visit to St. Augustine. His first real estate venture, construction of the Ponce de León, a luxury hotel in St. Augustine, quickly led him to buy his first railroad, the Jacksonville, St. Augustine, and Halifax River, an inefficiently operated and poorly maintained property that he improved to provide his hotel's northern guests with better service. The Ponce de León and the Jacksonville line served as the bases on which Flagler built two empires: a luxury hotel chain and the Florida East Coast Railway, organized in 1895 to combine the roads he had acquired on his drive southward, building hotels in Palm Beach and Miami. His greatest feat, extending the East Coast's route from Miami to Key West, through everglades and open sea, was not completed until 1912, a year before Flagler's death.

Akin appreciates the significance of Flagler's Florida enterprises, explains clearly the business strategies he employed, and incorporates enough of the state's social and economic history to give needed breadth to the hotel, railroad, and other development projects discussed. And he does not neglect the personal side of Flagler's life. Akin makes good use of it to explain the vision and ambition that turned a retired Standard Oil magnate in his mid-fifties into a Florida tycoon.

VINCENT P. CAROSSO
New York University

PATRICIA MOONEY MELVIN. *The Organic City: Urban Definition and Community Organization, 1880–1920*. Lexington: University Press of Kentucky. 1987. Pp. xii, 227. \$25.00.

This book chronicles the career of reformer Wilbur C. Phillips, who promoted the idea of "social unit" (or neighborhood) organization early in this century. Patricia Mooney Melvin's main project is to narrate Phillips's career and, along the way, to draw out middle-range generalizations about urban politics and reform.

Like many other reformers, Phillips was the scion of an evangelical minister, a college graduate, and an erstwhile journalist, whose career crisis ended in a commitment to social service. His successful work with the New York City clean milk campaign of 1906–07 and then with the Milwaukee Child Welfare Commission confirmed his belief in "interdependence"—of neighborhoods with each other, of the masses and the experts, of individuals and their environment. His "Social Unit Theory of Organization" envisioned a "cooperative society" in which the "public would define

the ends while the experts would devise the means" of social betterment (p. 58).

The great test of the theory came when Phillips was invited to the Mohawk-Brighton district of Cincinnati in 1917. A mixed industrial and residential neighborhood of second- and third-generation immigrants, Mohawk-Brighton eagerly welcomed an experiment that promised to reduce infant mortality and improve local health care. With few exceptions, doctors, nurses, social workers, teachers, civic boosters, and politicians cooperated to make the experiment a success. Block workers and citizens' councils identified health needs; occupational councils offered expert guidance; and professionals, especially nurses, collected vital information from and dispensed health care to almost the entire target population. The experiment won the praise and cooperation of the new federal Children's Bureau. The plan's very success, however, led to its downfall.

The coming of war, revolution, and Red Scare gave the plan's opponents an excuse to brand it a "bolshhevik" conspiracy. In truth, the social unit idea required what was in essence a parallel government, whose occupational councils and citizens' councils threatened the turf of established philanthropies and political organizations. The plan's future financing would have been in doubt under the best of circumstances, but the depth of support from residents and local helping professionals remained impressive to the end. Melvin suggests that the plan would have faced a more resistant environment as "metropolitan community" increasingly gave way to "fragmented metropolis" in the twentieth century. Clearly, Phillips's notions of community could not readily incorporate the brokering of conflicting interests, which was the *métier* of existing political organizations. It was the campaign of Red baiting, however, that sank the experiment and ended the potential threat to political and charitable elites.

I wish that Melvin had made Phillips's story speak to broader questions, especially the continued usefulness of notions such as "corporate liberalism" and the "therapeutic state" in understanding the Progressive era. It is also unfortunate that this story was not made to intersect with the new urban political history produced by authors such as David Hammack and Terrence McDonald. Even the book's coverage of the history of local politics, class relations, and ethnicity is thin. But Phillips's career is worth narrating and offers material for the research of others who will delve more deeply into the history of urban reform and politics.

FRANCIS G. COUVARES
Amherst College

LOUISE LAMPHERE. *From Working Daughters to Working Mothers: Immigrant Women in a New England Industrial Community*. (Anthropology of Contemporary Issues.) Ithaca: Cornell University Press. 1987. Pp. xviii, 390. Cloth \$45.00, paper \$14.95.

In this impressive and richly detailed book, Louise Lamphere, an anthropologist, "examines the transformation of women's roles in productive labor from that of a wage-earning daughter to that of a wage-earning wife" (p. 31). Combining the techniques of anthropological research with those of the historian, she reconstructs the history of women's wage work in a small Rhode Island town. Her aim is to illuminate the effects of women's work on their positions in the family and on their strategies of resistance in the workplace. Women, she concludes, have been active agents in both spheres.

The substance of the book is contained in two parts preceded by an extended theoretical framework. In the first part, the narrative traces the development of the textile industry in Central Falls, Rhode Island, from the early nineteenth century to about 1940. Using census records, oral histories, and local newspapers and focusing on the period from about 1900 to 1940, Lamphere documents the forces that drew unmarried daughters into wage work. These include the composition of ethnic and neighborhood groups, the changing structure of job opportunity for both men and women, and, as nearly as evidence permits, the dynamics of family values. Insofar as patterns of women's work are concerned, this material affirms and extends our previous knowledge, suggesting that commonalities among women overrode ethnic divisions in the struggle against management and that women were potentially militant workers. In several respects, however, the conclusions challenge current interpretations. For example, Lamphere argues that kin were less important than ethnic networks in obtaining jobs and that the place of an ethnic group within the local economy rather than distinctive ethnic values influenced who worked and where.

The second part brings the narrative to 1980 and adds Colombian immigrants to the Polish, British, Irish, Portuguese, and French Canadians who figured earlier. It also moves from women who are daughters to those who are wives and mothers. To gather data, Lamphere used Spanish- and Portuguese-speaking students, and she herself worked in a garment factory for several months. These techniques enable her to say far more about workplace dynamics and about family feelings than written sources permit. Her conclusions affirm those of the historical data with regard to the importance of the local economy, and,

again, they challenge some current wisdom. Lamphere notes that, despite a verbal adherence to traditional values, dual work roles have significantly altered the behavior of women and men (especially of men) within families. As in the earlier period, she depicts women as active agents in altering family patterns and in creating mechanisms for coping with uncomfortable work situations.

Although one can only admire the careful and complicated reconstruction and comparison of ethnic, family, and neighborhood patterns over time, Lamphere's depiction of the relationship between this experience and that of the workplace leaves many questions hanging. What difference did it make to workplace resistance whether women were daughters or mothers or wives? How is women's resistance at work fostered or hindered by family values? Is the location of women in families an appropriate place from which to begin an analysis of workplace behavior? What of the significant proportions of independent women in the past and of fluctuation and variation in women's domestic lives in the present? Can a "coping" strategy accurately be defined as a strategy of resistance? How much should we make of the fact that the history offered is that of a town built on textiles, while the most graphic pictures of contemporary resistance are drawn from the author's own work experience that occurred in a garment factory? Is the reader entitled to conclude that the kinds of strategies that evolve are independent of particular workplaces?

Lamphere has successfully demonstrated that women actively shape their family and work lives within the constraints of ethnicity and the local economy. Her accomplishment is enhanced by our capacity to use her data to advance the discussion.

ALICE KESSLER-HARRIS
Temple University

ELIZABETH ANNE PAYNE. *Reform, Labor, and Feminism: Margaret Dreier Robins and the Women's Trade Union League*. (Women in American History.) Urbana: University of Illinois Press. 1988. Pp. xiv, 218. \$24.95.

Elizabeth Anne Payne has written a fascinating analysis of Margaret Dreier Robins's leadership of the Women's Trade Union League (WTUL) between 1907 and 1922. In particular, Payne has illuminated the interweaving of progressivism, unionism, and feminism that produced the WTUL's unusual cross-class and cross-cultural alliance of women.

In many ways, Margaret Dreier Robins (1868–1945) perfectly exemplified her generation of fe-

male reformers. Reared in Brooklyn Heights, she was the favorite child of her father—a successful businessman—whose special attention and a fine education filled Dreier with aspirations for public achievement. Yet, like many of her contemporaries, Dreier found no obvious outlet for her abilities because society's expectations continued to exclude middle-class women from business, professions, and politics. Ambitions frustrated, Dreier suffered a long depression, which lifted in 1904–05 when she joined the newly organized WTUL and married the enigmatic Chicago reformer Raymond Robins.

The WTUL provided Robins an organizational base for a career in reform. Founded by settlement workers in 1904, the WTUL united middle-class women with their wage-earning sisters in efforts to unionize female workers. Elected president of both the National WTUL and its Chicago branch in 1907, Robins used her substantial wealth to set the fledgling organization on its feet and soon infused it with her singular blend of progressivism, unionism, and feminism.

To identify the unique space that the WTUL occupied in the mosaic of progressive reform organizations, Payne shows where Robins's ideology intersected with that of her contemporaries and where it diverged. For example, Robins allied with settlement workers to lobby for protective legislation for working women, to organize English classes for immigrant women, and to support striking garment workers. But Robins dissociated herself from the ultimate goals of settlement women: she disapproved both their reliance on bureaucratic solutions to industrial problems and their satisfaction with providing material sustenance for victims of the industrial system rather than striving toward the spiritual regeneration of society. Although Robins failed to specify the means to this social redemption, her insistence on moral ends and her suspicions of centralized bureaucracies set her firmly in a nineteenth-century reform tradition that would later render her hostile to the New Deal.

In other ways, too, Robins continued nineteenth-century traditions. Like the leaders of the Knights of Labor, she was committed to industrial unions that welcomed all workers—female and male, skilled and unskilled, immigrant and native-born—in the struggle to redeem the social order. (This inclusive reform unionism, of course, troubled Robins's relationship with the American Federation of Labor, devoted as it was to exclusive craft unions and what were, in Robins's view, narrow economic goals.) Furthermore, Robins's feminism was rooted in the nineteenth-century doctrine of separate spheres, which insisted on the fundamental difference between women and men.

Payne argues very convincingly that gender solidarity—based on “a distinct female culture reaching back to the 1830s” (p. 4)—allowed the WTUL to overcome its ethnic and class divisions. Indeed, Payne shines as she analyzes Robins’s “ideology of motherhood” and its role in shaping the WTUL’s policies, methods, relationships, and rituals.

For historians of the Progressive era, Payne’s book provides further evidence that nineteenth-century traditions did not abruptly disappear in 1900 but shaded into the new century to color the era of Progressive reform. Moreover, as historians of women increasingly agree that the New Deal represented the culmination of a female progressive reform movement, they will have to acknowledge women such as Robins, who considered the New Deal a betrayal of her version of progressivism.

ROBYN MUNCY
Le Moyne College

DANIEL A. CORNFORD. *Workers and Dissent in the Redwood Empire*. Philadelphia: Temple University Press. 1987. Pp. x, 276. \$29.95.

In the introduction to this book, Daniel A. Cornford argues briefly that historians of American society should slow their rush to pull a synthesis out of the tangled mass of community and workplace studies that have appeared since the 1960s. He maintains that several regions, particularly the West, have yet to receive the close analytical attention of scholars of labor history.

That statement serves to justify what is the first full-length study of work and politics in Humboldt County, California, a relatively isolated region in the northwestern corner of the state that was one of the nation’s major lumbering areas from the 1870s through the 1960s. Cornford focuses on the period between the end of the Civil War and the onset of World War I to understand how the rise and consolidation of the industry in Humboldt affected working-class organizations, ideologies, and, to a lesser extent, life on the job.

The result is a monograph that historians of western labor and politics will find valuable but one that should not cause advocates of a national synthesis to tarry. In the center of Cornford’s concise but nuanced narrative is a loose network of articulate, experienced working-class activists who campaigned, with intermittent success, to recruit members and control local government during the Gilded Age and Progressive era. Many of the same men moved sequentially from Greenbackism to the Single Tax to the Knights of Labor to Populism to socialism, carrying with them a faith in republican ideals and a determination to elect

public officials who would fight for the needs of wage earners and small businessmen and against those of the “moneyed aristocracy” (p. 125).

Trade unions, facing an unequal struggle against lumber companies whose commitment to the open shop deepened as their operations and wealth expanded, enjoyed far less continuity and success than did the political vehicles that championed their cause. The authoritarian labor policies of the new, nonresident owners who came to dominate the industry by the first years of the twentieth century sparked a wave of union activity. But employers used the mechanisms of their power—abundant sources of outside capital, an extensive railroad network to export lumber and import workers, and company towns—to crush all attempts to organize the labor force. Countering earlier studies that tended to “romanticize” lumber workers “and to portray them as both militant and excellent union material” (p. 246), Cornford correctly emphasizes the disunity and desperation that plagued all organizing attempts until the 1940s. He thus contributes to a growing body of rather sober studies that prefer to document and interpret the limits of labor’s power rather than to celebrate what E. P. Thompson called the “heroic culture” of working men and women.

Unfortunately, however, Cornford does more to confirm what labor historians already know than to force avid synthesizers to reexamine their premises. Although he is aware that the same language can have both radical and conservative uses, Cornford, in his discussion of working-class republicanism, echoes the arguments of such historians as Herbert Gutman, David Montgomery, and Nick Salvatore. His narrative of industrial conflict and union defeat resembles those written about company towns of coal miners, copper miners, and textile workers elsewhere in the country. And his meticulous tracing of dissenting organizations and their leaders neglects to explain why such alternatives were more popular in Humboldt County than in other parts of the rural West. Cornford offers much material for thought. But he has not drawn the connections between a local study and broader issues that would make his book essential rather than merely useful.

MICHAEL KAZIN
American University

HARVEY A. KANTOR. *Learning to Earn: School, Work, and Vocational Reform in California, 1880–1930*. Madison: University of Wisconsin Press. 1988. Pp. xvi, 240. Cloth \$39.50, paper \$18.95.

This book is a study of the development of vocational education in California secondary schools

between 1880 and 1930. Harvey A. Kantor begins with two chapters that provide a solid survey of the origins of vocationalism nationally, and then he moves on to a careful examination of the California case. The result is a book that offers a coherent analysis of both the broader social and educational issues involved in this reform and the particular contingencies that shaped its development in a single state.

Kantor shows that the pressure for establishing vocational education came primarily from educators (rather than industrialists) who sought to accomplish two contradictory goals: to promote economic development by adapting the school to the needs of the economy and to encourage potential dropouts to stay in school through the promise of job-relevant training. This meant, on the one hand, subordinating the school to the economy, while, on the other hand, using the economy to draw students into an involvement with the broader social and political aims of the school. The irony, according to the author, is that vocationalism was not very successful in achieving either of these goals, but it was quite successful in helping to reorient public education toward the principle of economic instrumentalism that underlay both. Its ultimate legacy was in reconstructing the rationale for education as, in the words of the title, "learning to earn."

Kantor's primary contribution—and it is a considerable one—comes in his cogent and convincing analysis of how and why vocational education was deflected from its original target. The problem was that vocational educators misread both the nature of the job market and the aspirations of their clientele. Reformers heavily promoted industrial education for boys and home economics for girls, but neither proved to be nearly as popular as the traditional academic course or the one vocational program that came close to rivaling it, the commercial course. Parents and students were better aware than educators that, as fewer skills became necessary to industry, vocational training became largely irrelevant to the process of landing a factory job, while the expanding clerical sector tended to hire applicants with the kind of typing and stenographic skills provided by the high school commercial course. Students actively resisted the attempt by vocational educators to confine them within a track that could lead only to working-class jobs, since the opportunity to attain a more desirable clerical or professional position was accessible only through the commercial and academic courses.

As a result, vocational education institutionalized a system of curriculum tracking that separated knowledge into mental and manual categories and led to differential class outcomes, but it

did so within a comprehensive high school format where alternatives remained at least formally open. It helped adapt schools to the job market, but it did so by serving the status aspirations of high school students rather than the perceived need in industry for disciplined and skilled manual workers. And ultimately educators managed to adapt vocationalism to their own nonvocational ends, as vocational guidance counseling within high schools gradually became transformed into a mechanism for handling discipline problems, course scheduling, and testing. Kantor's book provides a clear explanation of this mixed legacy of vocationalism in American education.

DAVID F. LABAREE
Michigan State University

WILLIAM J. REESE. *Power and the Promise of School Reform: Grass Roots Movements during the Progressive Era.* (Critical Social Thought.) Boston: Routledge and Kegan Paul, in association with Methuen, New York, 1986. Pp. xxx, 342. Cloth \$39.95, paper \$14.95.

My first thought about the publication of still another book on school reform during the Progressive era and its social, economic, and cultural ramifications was why? Surely the topic, incessantly studied by social historians during the past quarter-century, has had its kernels of historical significance sufficiently cultivated, harvested, and distributed.

William J. Reese's volume belies this notion because of his systematic use of novel sources that inexorably lead us beyond the thesis that has dominated historiography in this field: that school reform during the Progressive era was imposed from the top by centralized school boards composed of community elites bent on "social control" of the young (and, by implication, of the classes and cultures from which the young sprang).

Although not denying the power wielded by elitist school boards, Reese focuses on the values and actions of a heterogeneous array of "grass-roots" reformers—Social Gospelers, socialists, women's groups—who actively struggled against the potential hegemony of elites and fostered a series of countervailing images of the means and ends of education as well as the social utility of the schools' physical plants. Reese traces the struggles between these groups and local elites (as well as the differences among the grass-roots groups) from the 1880s through World War I in four cities: Rochester, New York; Toledo, Ohio; Kansas City, Missouri; and Milwaukee, Wisconsin. Within the context of significant differences in the political, social, and ethnic contours of each city, Reese

presents the ways in which workers and socialists, women's groups, and Christian socialists attempted to mitigate elite control of schools by creating a series of school functions—vacation schools, playgrounds, social centers, the school health movement—that in one fashion or another contradicted the capitalist and, not infrequently, nativist presuppositions of centralized school boards.

For many reasons Reese's book is an outstanding, path-breaking contribution to the fields of both Progressive and educational historiography. Two reasons are especially significant. First, Reese resuscitates the lives and values of scores of forgotten activists whose collective, if disparate, hopes of realizing a better America through school reform were just as compelling and, in some instances, just as viable as those of the elites who controlled school boards. Second, by presenting the complex issue of school reform as a dialectical process in which elites hardly attained hegemony and the "masses" were far from passive in designing distinctive images of the utility of schooling, Reese's volume presents the struggle over control of the schools in a manner far more sophisticated than the "social control" historians, whose emphasis on the quest of reformers to "control" the young has veiled the vital issue: control by whom for what ends?

To be sure, there are problems with Reese's approach. For example, he does not confront the question of how the varying approaches to schooling as a social function was related to the abject failure to produce effective curricula. Indeed, there is practically no discussion of curricular issues in the book.

Nonetheless, Reese has done an outstanding job of restoring complexity and nuance to the study of educational history during the Progressive period.

DOMINICK CAVALLO
Adelphi University

JONATHAN LIEBENAU. *Medical Science and Medical Industry: The Formation of the American Pharmaceutical Industry*. (The Henry E. Sigerist Series in the History of Medicine.) Baltimore: Johns Hopkins University Press. 1987. Pp. ix, 207. \$35.00.

Effective medical care would be hard to come by without the role played by the modern pharmaceutical industry, so this part of the American industrial development and its underpinnings in science and technology merit historical assessment. Jonathan Liebenau's monograph focuses mainly on the four decades from 1890 to 1930 because within this time frame the elite firms of the industry institutionalized scientific investiga-

tion as an understood part of their role and mission. The author attempts, not without difficulties, to distinguish between those manufacturers who became committed to in-house pharmaceutical science as a legitimate use of industrial capital and those who were more interested in exploiting the verbiage of science as a promotional ploy.

The scope of the book is conceived more narrowly than is implied by the main title. "Medical science" here alludes mainly to sciences applicable to pharmaceutical problems, with an overemphasis on biosciences, and "medical industry" alludes mainly to large-scale production of products intended primarily for prescription use (but not including medical devices, surgical instruments, sickroom supplies, so-called patent medicines, and so on). Moreover, the medical industry discussed is predominantly Philadelphian industry, which indeed was the principal center of early production (partly because Philadelphia was in general a center of American activity in medicine, science, and technology). But that leaves us wondering about the comparability of conditions in companies that later sprang up elsewhere, not only the major producers (such as Eli Lilly and Company and E. R. Squibb and Sons, which have good archives) but also the myriad of small regional producers across the country.

A merit of this study is that it fulfills the promise of its subtitle. The author offers us more than the minimum context needed to understand the interactions between science and those firms chosen as exemplars of the pharmaceutical industry, without attempting a history of the industry as a whole.

The book is well documented and supplemented by a bibliography, although there are too many misprints. On page 155, for example, the author's name is Stechl (not Steckl), Scoville (not Schoville), Rusby (not Rustby); the first U.S. Pharmacopoeia was not published in Easton; and the second and third revisions were not "issued" in 1840 and 1850. The narrative achieves much better precision, although some questions could be raised about the meanings of a few terms, ambiguities of interpretation, and topical balance. For example, what is the support or import of the statement that the chain drugstore movement and its implications constituted a trend that "finally separated out those companies which were to concentrate on wholesaling and producing proprietary medicines from those which based their business on ethical pharmaceuticals" (p. 133)?

A definitive history has not been achieved, and probably no historian could have done so in 133 pages of text, especially when existing secondary works are so sparse. The author thus had to dig into a wide range of primary sources to attempt

the first book-length synthesis. Liebenau's book should be consulted before Tom Mahoney's *Merchants of Life* (1959), which is undocumented and smacks of a company history. More recently, Liebenau's book has been supplemented by John P. Swann's excellent *Academic Scientists and the Pharmaceutical Industry* (1988).

GLENN SONNEDECKER
University of Wisconsin

JOHN P. SWANN. *Academic Scientists and the Pharmaceutical Industry: Cooperative Research in Twentieth-Century America*. Baltimore: Johns Hopkins University Press. 1988. Pp. xi, 249. \$32.50.

This is a pioneering study of the symbiotic relationships between drug manufacturers and university-based laboratory or clinical investigators that first flowered between the two world wars. John P. Swann's assertion that it is an "interpretive rather than a comprehensive study" (p. 2) is probably true, but such a pathfinding work does not really need to include all possible data. His selection of case studies for detailed analysis does indeed provide a satisfactory survey of the academic-industrial interactions that led to a number of major therapeutic innovations between 1920 and 1940. At the same time, Swann's story implicitly refutes the commonly held idea that drug companies were unconcerned with patient-oriented factors such as drug safety and efficacy before the 1938 and 1962 amendments to the Food and Drug Act required that both be taken into account.

Although industrial research had begun during the nineteenth century, it burgeoned significantly after World War I (it would, however, have been instructive to learn how fast the pool of research workers with M.D.'s and Ph.D.'s was growing at the same time). The pharmaceutical industry first called on academic scientists to help standardize drugs. One barrier to further academic collaboration was a ban of industrial scientists from membership in the American Society for Pharmacology and Experimental Therapeutics (in effect from the society's founding in 1908 until 1941). Another was the American Medical Association's prescription in 1847 against the award of medical patents to its members.

By the time such attitudes began to change after the First World War, the full impact of the federal Food and Drug Act of 1906 was being felt in the drug industry, and it came to rely more heavily on patent protection for its products. At the same time, American medicine and medical education were becoming more systematic and more scien-

tific. The time was ripe for fruitful collaborations for mutual benefit.

Swann identifies three generic types of academic consultants to the drug industry, although they were not mutually exclusive. The rarest were the "general consultants" who not only were eminent scientists but also had wide contacts that they could exploit while helping to direct a company's overall research policy. The "specialist consultants" were usually biomedical scientists who used their knowledge of broad fields such as pharmacology or microbiology to guide a company's efforts toward promising drugs or drug classes. Finally, the most numerous were the "project researchers" who collaborated in developing and testing specific new drugs; physicians seem to predominate in this category, at least in Swann's sample.

Sometimes academic consultants were paid personally, but more often the drug companies who employed them provided financial support for their university laboratories, especially in the form of fellowships for graduate students who participated in that part of the laboratory's work devoted to the shared projects. The remuneration might be paid from the company's general income, but as patents became more important and more rewarding the researchers were given royalties from the sales of products they had either invented or studied for the companies.

This story may well have analogues worth exploring for other industries. It should be of interest to students of American business, science, and education. Several of the drug development stories told here center on capsules and injectable drugs found in many households today, although some are about drugs whose usefulness has passed, and a few are about drugs that academic consultants found to be unsafe before they could be marketed. Swann is reasonably successful in explaining those technical points that are essential to his story (although my own background in pharmacology and hematology and as a "project researcher" certainly facilitated my ability to understand each case history). The few typographical errors are only slightly annoying in this otherwise well-written volume, and the index is adequate.

J. WORTH ESTES
School of Medicine,
Boston University

ROGER A. FISCHER. *Tippecanoe and Trinkets Too: The Material Culture of American Presidential Campaigns, 1828-1984*. Urbana: University of Illinois Press. 1988. Pp. x, 322. \$34.95.

Addressing himself to both collectors and historians in this amply illustrated survey of the artifacts of presidential campaigning, Roger A. Fischer engagingly crowds extensive detail into a coherent narrative on the material culture of American political history. His contribution should please his intended readers. Helpful endnotes and a perceptive assessment of sources increase the usefulness of his encyclopedic account. Space limitations necessarily excluded a consideration of state and local campaigns as well as inclusion of biographies, song sheets, broadsides, and cartoons, categories that Fischer nevertheless believes "deserve more attention from the scholarly community" (p. xi).

Fischer's engrossing study does more than resurrect trivia and arouse nostalgia. Political culture is revealed in symbolism inherent in a "Cincinnatus," a "Railsplitter," a hole-in-the-sole lapel pin; in log cabins, full dinner pails, and gold bugs; and in political issues presented in the simplistic economy of bumper stickers, paper earmuffs, "mechanical nose-thumpers" (p. 196), and thirty-second radio and television spots. Changes in electoral constituencies and in technologies prompted changing materials. Dishes and ceramics had been popular long before women were enfranchised, but, when women got the ballot, thimbles, sewing kits, and needle packs were added to a list that included flasks, watch fobs, snuff and cigar boxes, and other smoking paraphernalia that candidates distributed to herald their cause. Automobiles brought emblazoned tire covers and bumper stickers. Innovation in textiles brought printed and painted T-shirts.

Readers will not be surprised to find that politicians can be inane, scurrilous, and self-serving as well as ingenious. They enjoy word play and tedious repetition. The echolalia of William Henry Harrison's campaign in 1840 re-echoed in his grandson's campaign in 1888: "Tippecanoe and [Levi P.] Morton Too." A "Nebraska Cuckoo Clock" with the face of William Jennings Bryan would "run every four years if properly wound" (p. 179). Followers of Henry Clay enjoyed repeating the shibboleth "The Same Old Coon," seemingly providing the Kentuckian with a "down-home identity" (p. 58). At the trivia level, artifacts revive memories of forgotten candidates such as Henry Gassaway Davis, "the octogenarian running mate" of Democrat Alton B. Parker in 1904, and minor party champions like Prohibitionist Silas Comfort Swallow, whose name inspired multicolored buttons "featuring bird word plays" (p. 175). Scurrility reached its nadir when racist opponents of Theodore Roosevelt exploited his White House dinner with Booker T. Washington in language and doggerel too shameful to repeat.

As campaigns "evolved inexorably into meticu-

lously staged media events," the persuasive importance of artifacts "diminished steadily in importance" (p. 234). Less "emphasis on participatory activities at the local level" contributed to this decline. Recently, Fischer concludes, a "spirit of introspective detachment" has "grown so pervasive that even intensely committed partisans" become "less likely to flaunt their beliefs with buttons and other devices" (p. 235). Besides, pin-backed buttons often damage polyester clothing, thus further diminishing their popularity.

Social psychologists and quantifiers have yet to assess convincingly the persuasive effectiveness of trinkets, but Fischer has supplied these specialists with a lively and exhaustive history of them.

ROBERT G. GUNDERSON
Indiana University

LLOYD E. AMBROSIOUS. *Woodrow Wilson and the American Diplomatic Tradition: The Treaty Fight in Perspective*. New York: Cambridge University Press. 1987. Pp. xvii, 323. \$34.50.

Although this work has the form of a monograph, it is really a sermon—relentless, even repetitive—on the virtues of realism in foreign affairs. Like N. Gordon Levin and others, Lloyd E. Ambrosius sees Woodrow Wilson as a paradigmatic figure, representative (and in some senses creator) of an "American diplomatic tradition" that has led us astray. Wilson simply did not understand that "pluralism"—a code word used repeatedly to mean differing national interests—is a fact of international politics. Except in rare moments of despair, Wilson believed American values so transcendently superior that they could be imposed on the world, primarily through the agency of the League of Nations. In Wilson's view, Ambrosius writes, the League "would allow the United States to provide global leadership without entangling itself in the Old World's diplomacy and wars" (p. 290). (Too often, Ambrosius substitutes the words "hegemony" and "control" for "leadership.")

With his schema very much in mind, Ambrosius traces the growth of Wilson's commitment to the League and his successful fight to shape the Covenant and make it part of the Treaty of Versailles. Ambrosius often turns to other issues at Paris, and the chapter dealing specifically with the Covenant is paradoxically both brief and dense, but the moral he draws is clear. Often successful tactically, President Wilson suffered inevitable defeat at a higher level: he "failed to transform international relations in accordance with his ideals. This failure resulted less from the inadequacy of his diplomatic skill than from the impossibility of the task.

No statesman could achieve the democratic redemption of the Old World. That unrealistic goal exceeded the means available to the United States" (pp. 124-25).

The second half of the book describes the American battle over the treaty. "Advocates and critics," Ambrosius says, "disagreed basically over the question of whether the League would enable the United States to control the world, or the world to control the United States" (p. 136). Central, of course, was disagreement over Article X of the Covenant. Wilson, unable to conceive that the League council would act contrary to American values, steadfastly opposed efforts to qualify the commitment in that article; opponents, lacking his confidence, insisted that the nation must be free to reject the council's advice. Between these views, no compromise proved possible.

Ambrosius argues that Henry Cabot Lodge wanted the United States to join the League. Lodge, he says, "saw [Wilson] as 'an awful bluffer' and expected him eventually to compromise. Anticipating that contingency, Lodge wanted strong reservations to protect American interests and also to create the opportunity for ratification" (p. 190). Relying heavily on the senator's correspondence with pro-League Republicans, Ambrosius overstates the case made more moderately by William C. Widenor and others.

In the tone of Thomas A. Bailey, Ambrosius assigns to Wilson primary responsibility for American rejection of the League. "If leaders in other countries or Senators in the United States rejected his particular conception of global interdependence, as they did, the President denounced them for preventing the United States from playing a constructive role in the world," Ambrosius writes. "Yet [by refusing all compromise] he was the person most responsible for American rejection of the Versailles peace" (p. 250). There is, of course, merit to this argument, but it is not the whole story.

The volume closes with a strained epilogue that seems to blame Wilson, or at least Wilsonianism's inability to respect "pluralism," for misfortunes from the aftermath of World War I to Henry Kissinger's time. This is an unfortunate conclusion to a book that presents a forceful argument and adds some fresh information on an important act in American history.

BRADFORD PERKINS
University of Michigan

MICHAEL P. WEBER. *Don't Call Me Boss: David L. Lawrence, Pittsburgh's Renaissance Mayor*. Pittsburgh: University of Pittsburgh Press. 1988. Pp. xxii, 440. Cloth \$32.95, paper \$16.95.

David L. Lawrence has a dual distinction in American history; he was one of the last of the old-time bosses and one of the first of the renaissance mayors. He allocated patronage to ward leaders in the time-honored tradition, but he also won plaudits from the business elite for his cooperation in the program to renew Pittsburgh. He was, then, a man who bridged the distance between the old politics and the new, a masterful figure well deserving of Michael P. Weber's excellent biography. In 391 pages, Weber traces Lawrence's rise as leader of Pittsburgh's Democratic party, his emergence as a power in national party circles, his years of service as mayor, and his term as governor of Pennsylvania. Weber's study offers an admirably thorough account and will probably be the definitive work on the Pittsburgh boss.

Weber begins his survey with two chapters on Lawrence's career prior to the political transformation of the 1930s. As a protégé of long-time county chair William Brennen, Lawrence inherited local leadership of the Democratic party when he was only thirty-one years old. The position, however, bestowed little power on its holder, for Pittsburgh in the 1920s was solidly Republican territory. Traditionally the Democratic organization had won some patronage crumbs by cooperating with the Republicans, and Lawrence continued to maintain beneficial ties with the GOP. The Republican county chair was his partner in the Harris-Lawrence Insurance Agency, and another local Republican boss was one of his closest friends. Though a fervent Democrat, Lawrence was above all a pragmatist. In the 1920s cooperation with the Republicans was in the best interest of the Democratic organization, so that was the course Lawrence pursued.

Economic depression and the New Deal suddenly transformed Lawrence from a minor figure on the local political scene to a kingmaker. Within a few years Pittsburgh shifted from Republican to Democrat, and, from the late 1930s on, the mayor's office and all of the city council seats remained in Democratic hands. Meanwhile, Lawrence became state party chairman and served as secretary of the commonwealth in the administration of Governor George Earle. Lawrence's foray in state politics resulted in his indictment for corruption and two trials that ended with not-guilty verdicts. Despite this favorable outcome, the indictments tarnished Lawrence's record and momentarily damaged his career.

By the mid-1940s Lawrence had recovered his political stamina and was ready to serve as Pittsburgh's chief executive. As mayor presiding over the so-called Pittsburgh renaissance, Lawrence won both local and national acclaim and solidified his political clout. During the two decades after

World War II, he played a major role at the Democratic national conventions and proved an especially dedicated admirer of Adlai Stevenson. At the age of sixty-nine, Lawrence capped his already illustrious career by capturing the office of Pennsylvania governor where he fought for a balanced budget, highway safety programs, and educational reform.

Weber capably recounts the details of this long and distinguished career, and his enthusiasm for his subject is undisguised. In his preface Weber says that he "admittedly came to admire" Lawrence (p. xii), and at times the reader may find this an understatement. The author excuses faults and lavishes praise to an extent that is occasionally irritating. In the pages of Weber's study, Lawrence's enemies seem to have few redeeming characteristics; political opportunism, spite, and pettiness generally appear to be their only motives for challenging the boss. Lawrence, on the other hand, acts unselfishly on behalf of his twin loves, the Democratic party and the city of Pittsburgh.

This study is, then, a work that should appeal especially to Lawrence fans. Yet Weber's volume merits the attention of a broader audience. Anyone interested in twentieth-century urban America, Pennsylvania history, or the Democratic party will benefit from reading this book.

JON C. TEAFORD
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MICHAEL A. BERNSTEIN. *The Great Depression: Delayed Recovery and Economic Change in America, 1929-1939*. (Studies in Economic History and Policy; The United States in the Twentieth Century.) New York: Cambridge University Press. 1987. Pp. xvii, 269. \$29.95.

Analyses of the Great Depression of the 1930s divide, in part, over the relative importance of long-range, or "secular," and short-range, or immediate, factors. Michael A. Bernstein, in this study of the duration of the depression in America, stresses the role of secular forces. He also, however, gives prominent attention to what he considers a basic immediate source.

Specifically, Bernstein focuses on the state of newer growth industries in the economy. At the time the stock market crash hit, and the depression ensued, he argues, these newer industries were not yet well enough developed to pull the economy out of a downward spiral. They were not in a position to absorb massive numbers of unemployed and thereby to increase aggregate demand sufficiently to the point of achieving recovery. This situation, in part, accounted for the depression's exceptional duration.

Although stressing these secular factors, Bernstein also examines an immediate one, the crash of 1929. The crash so disrupted the financial workings of the economy, he argues, that it underlay the inability of secular development to proceed. Drastic deflation, reduced demand, and failed confidence, emanating from the crash, kept the doors closed to renewed growth until World War II. It was through these combined long- and short-range factors that a "cyclical" downturn became a prolonged depression. It is here that Bernstein's argument appears to run into difficulty. On the one hand, he explicitly disavows any concern with the causes of the depression; his study is of its duration. On the other hand, his stress on the impact of the crash is so strong and so central that it raises a question about the appropriateness of this disavowal. In fact, he seems to assume a cyclical downturn as given, while combining short-range and long-range explanations for its duration.

In his consideration of alternative explanations of the depression's length, Bernstein gives short shrift to other secular theories, such as the institutional analysis that focuses on the growth of oligopoly, "administered prices," and income maldistribution as well as the Keynesian-Hansenian analysis that centers on "economic maturity" and "stagnation." He relies on negative critiques of these and other theories and rejects them after only brief review, on the grounds of logical or evidential flaws. The only secular theory he tests against the evidence is his own hypothesis (which is somewhat reminiscent of Joseph Schumpeter's). This lack of full consideration of other secular analyses prevents Bernstein from effectively relating his own "new industries" thesis to other secular developments, including changes in economic structure and environment. He does allow, however, that a pre-crash "skewed distribution of income" (p. 173) impeded economic growth. One has the impression that Bernstein started with a study of the industrial state of development as it pertained to the depression's persistence rather than with an effort to account for that persistence. The result, accordingly, is more satisfactory as an analysis of the impact of secular industrial change on an economy in crisis than as a full account of the causes of the depression's exceptional length and severity.

Still, whatever its limitations, the book does provide a refocus on secular forces in the Great Depression—forces often lost sight of in the heady decades of prosperity after 1940, only to be reencountered (as the author observes) in recent times.

THEODORE ROSENOF
Mercy College

ROBERT A. BURT. *Two Jewish Justices: Outcasts in the Promised Land*. Berkeley and Los Angeles: University of California Press. 1988. Pp. 165. \$19.95.

Writing books about Louis D. Brandeis and Felix Frankfurter has become a thriving cottage industry. The two Supreme Court justices have attracted, between them, more than a dozen biographers. There are now four joint biographies, from the adulatory (Leonard Baker) to the accusatory (Bruce Allen Murphy). The contribution of Robert A. Burt to this growing pile is the shortest (just 129 pages) and the most provocative.

What prompted this extended essay was Burt's curiosity at the predominance of Jews on the faculties of Yale and most other prestigious law schools. Jews now constitute almost one-half of the legal professoriate at the leading schools. The question of why so many Jews, as ethnic outsiders, "have found a home in America as lawyers" and law teachers (p. 2) led Burt to examine the roles of Brandeis and Frankfurter as Jews, as Zionists, as lawyers, and as justices.

The book's most striking feature is the disparity of Burt's feelings (a more personal word than attitude) toward the two men. He admires Brandeis to the point of idolatry, and he obviously detests Frankfurter. Brandeis is the "prophet" whose "instinctive sympathy with oppressed outsiders" (p. 28) infused his judicial opinions with a creative tension "between individualism and communal duty" (p. 13). Frankfurter, on the other hand, was a "priest" and "insider" who was "unsympathetic, uncomprehending, dismissive" (p. 43) of outsiders and who shed his early radical leanings to become "an overeager apologist for the existing order" (p. 60). The book's subtitle has a Mosaic ring; Burt, in fact, compares the two Jewish justices to Moses and his lieutenant Aaron. While Moses, who was confident in his faith, journeyed to Sinai for the tablets that revealed God's principles, Aaron, who lacked confidence, erected a golden idol in response to popular demand (p. 125). Burt's biblical lesson has a jurisprudential moral: Brandeis's opinions looked to first principles and expressed skepticism of legislative hostility toward outsiders, while Frankfurter's brand of "judicial restraint" reflected an unquestioning deference to Yahoo legislators, willing to write their constituents' prejudices into law.

The most satisfying parts of Burt's book are his insightful analyses of judicial opinions of Brandeis and Frankfurter, each related to deeper roots of personality and social role. The probings of Brandeis's opinions in the *Coronado Coal* and *Duplex Printing* cases, in which Burt relates them to Brandeis's identification with striking miners and printers as outsiders, are lapidary. Equally insightful,

and savage, is Burt's dissection of Frankfurter's opinions in the *Gobitis* and *United Mine Workers* cases. In the *Gobitis* case, Frankfurter vented an insider's scorn for the young Jehovah's Witnesses who objected to mandatory salute of the flag on religious grounds; in the *United Mine Workers* case, he posited against striking miners the power of judges (with himself in mind) as "the depositories of law" who were free of "the usual temptations of private interest" (p. 47).

Burt eschews any psychologizing about his subjects, viewing them less as individuals than as social types. He dismisses H. N. Hirsch's diagnosis, in *The Enigma of Felix Frankfurter* (1989), of the justice as an overcompensating "neurotic," although Frankfurter cries out for analysis of links between psyche and judicial behavior. Burt also overdraws his portrait of the United States as a country of universal alienation; his claim that "homelessness"—which Brandeis turned to compassion and Frankfurter twisted into callousness—is "the shared status of all Americans" (p. 74) robs his analysis of the insider-outsider conflict of explanatory force. But this short book is packed with insight and provocative social speculation.

PETER IRONS
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JILL QUADAGNO. *The Transformation of Old Age Security: Class and Politics in the American Welfare State*. Chicago: University of Chicago Press. 1988. Pp. xiii, 253. \$27.50.

This book seeks to explain why the United States was tardy (compared to most industrial societies) in developing a federal system for old-age security and to determine why America's welfare programs have remained market-based. Jill Quadagno agrees with many neo-Marxists that "the social organization of production determines the nature and form of relief programs" (p. 6). Since at least the sixteenth century, welfare programs have sustained the needy and allocated labor. But archival forays led the author to challenge the premises of those who attribute the rise of welfarism to the demands of a modernizing, capitalist political economy and of those who advance a social democratic thesis. Using a method Charles Tilly called "variation finding," Quadagno contends that at least three factors account for the "exceptionalism" of the American case. First, program development was hindered by the late unionization of mass-production workers. Second, private-sector initiatives have had a major impact on the timing, nature, and structure of public-sector benefits. Third, the dualism of the U.S.

economy, wherein labor and capital waged battle in the industrial Northeast and the planter class in the South maintained a plantation mode of production well into the twentieth century, delayed the growth of a national welfare state.

This book, like Brian Gratton's *Urban Elders* (1986), provides rich insights into issues largely ignored or only briefly mentioned in the first surveys of old-age history. No other monograph has yet so fully probed how organized labor's support of federal old-age pensions was tied to recruiting and satisfying union members. The antistate stance of some craft workers before the enactment of social security in 1935 has parallels, Quadagno shows, in the factionalism within the labor movement as it participated in setting a postwar agenda. She ably demonstrates, moreover, the importance of regional variations and racial distinctions. Others have alluded to the regrettable racism that pervaded welfare discussions in the 1930s and have referred to fears people raised at the time about the adverse effect federal initiatives might have on workers' wages. Quadagno's case study of the South illustrates why such matters must be probed at the state and local levels. Finally, her distillation and analysis of classic studies by Murray Webb Latimer and Luther Conant on private pensions before social security make essential data more accessible.

Although Quadagno has convincingly laid out important pieces of the puzzle, she has not fully made the necessary linkages among politics, economics, and welfarism, particularly in the South. "Class" merits more attention in historical gerontology, but it is surprising that women, who probably represented (and still do) the most vulnerable class of "dependent" workers, receive so little attention here. And I wonder whether Quadagno's model would be as effective in explicating the evolution of elder health-care policies as it is in explaining income-security matters. Nonetheless, her book offers important conceptual guidelines for future work.

W. ANDREW ACHENBAUM
University of Michigan

BARBARA S. GRIFFITH. *The Crisis of American Labor: Operation Dixie and the Defeat of the CIO*. Philadelphia: Temple University Press. 1988. Pp. xvi, 239. \$29.95.

The massive southern organizing drive, Operation Dixie, launched by the Congress of Industrial Organizations (CIO) in May 1946, generally receives but a few sentences in histories of American labor, and the monographic literature on it is slight. But, as Barbara S. Griffith points out,

Operation Dixie was a pivotal event for post-World War II labor and politics. If the CIO had succeeded in unionizing southern industry, the labor movement would have been much strengthened and regional wage differentials reduced, diminishing the incentive for runaway shops. In all likelihood the national and local political power of southern conservatives would have declined, and a major struggle for racial equality would have begun nearly a decade before the Montgomery bus boycott. Griffith's study of Operation Dixie is thus a welcome event for all those interested in the recent American past.

After preliminary chapters on the CIO and on southern labor, Griffith reconstructs Operation Dixie during its first, most intensive phase. Although the drive began with much hoopla, it quickly became evident that things were not going well. Southern workers generally were not receptive to the two hundred fifty organizers that the CIO put in the field. Often CIO representatives failed even to establish in-plant committees to carry on organizational activities. Unexpected CIO defeats in recognition elections at three North Carolina textile mills in August 1946 proved to be a portent. By November it was obvious that the CIO had failed to crack the textile industry, the foremost target of the drive. Some success was achieved in the tobacco industry and such northern-dominated industries as auto, steel, and meat packing. In the huge woodworking industry, however, the results were also dismal. In response, in late 1946 the CIO drastically scaled down its effort. Although Operation Dixie limped on until 1953, it never succeeded in organizing a significant number of southern workers.

Griffith devotes over half of her short book to a consideration of why Operation Dixie failed. Successive chapters deal with the impact of race, legal and extralegal harassment, religion, and Left-Right factionalism within the CIO. In each case, she presents abundant evidence of the problems the CIO faced but is cautious in her conclusions. She shows, for example, that antiunion religious publications and ministerial activities were widespread but notes that CIO organizers disagreed about their efficacy. More than any one factor, Griffith believes, it was the totality of the southern social structure, especially employer access to institutions of authority, combined with the CIO's inability to establish its cultural legitimacy that led to labor's defeat.

Griffith's study is primarily written from the point of view of the Operation Dixie organizers, many of whom she interviewed. Except in a chapter detailing the CIO effort at Cannon Mills in Kannapolis, North Carolina, the voices of rank-and-file workers rarely appear. Her treatment of

CIO decision making is also spotty. Griffith argues that Operation Dixie was modeled—in her view inappropriately—on the northern unionizing drives of the 1930s, in which “bellweather” companies were targeted in an effort to achieve galvanizing “breakthrough” victories. But, unlike most of those drives, Operation Dixie was highly centralized, bureaucratic, and deliberately divorced from broader social struggles. Griffith perhaps underestimates the importance of these factors in the drive’s failure. In explaining the CIO decision to curtail its effort, she stresses cost. But a CIO membership assessment of less than one dollar per year could have kept Operation Dixie going indefinitely. More basic may have been a lack of political will to promote the virtual social revolution that would have been necessary for Operation Dixie to have had a chance of success.

Griffith’s book is not a definitive study of Operation Dixie; that was not her intention. Rather, she set out “to open up the topic by setting in place the broad historical framework . . . within which the men and women of the CIO and their corporate opponents lived through the daily realities of the struggle” (p. xiv). In that she is successful.

JOSHUA B. FREEMAN
Columbia University

P. SCOTT CORBETT. *Quiet Passages: The Exchange of Civilians between the United States and Japan during the Second World War*. Kent, Ohio: Kent State University Press. 1987. Pp. viii, 226. \$22.00.

War with Japan brought the internment of thousands of American civilians in Japan and Japanese-occupied areas of Asia as well as the internment of thousands of Japanese nationals in the United States. How Japan and the United States worked out two exchanges involving several thousand people and failed to exchange thousands more is a complex story involving intricate diplomacy, bureaucratic rivalry, conflicting views of national security, and differing ideas of the rules of “civilized” warfare. P. Scott Corbett examines a narrow aspect of this story, the operations of the Special Division, a small office within the Department of State responsible for the exchange of those interned.

Although the Special Division was not a very important agency in World War II, the assignment brought it into contact and conflict with other government agencies, including the Office of Naval Intelligence, the Military Intelligence Division, the Federal Bureau of Investigation, and the War Relocation Authority. To his credit, Corbett has gone beyond the Special Division’s diplomatic negotiations with Japan and examined its

involvement with the internment of Japanese-Americans, the transfer to the United States of Japanese interned in Latin America, and the treatment of American internees held by Japan. In fact, only three of eleven chapters are concerned with the negotiations for exchange of civilians. Thus, the subject offers a good opportunity to study policy formulation and implementation in a variety of areas.

The usefulness of this work, however, is limited by Corbett’s decision not only to write the story from the perspective of the Special Division but also to rely almost exclusively on the records of that division and the papers of Assistant Secretary of State Breckenridge Long, who oversaw it. Corbett acknowledges that by not consulting Japanese sources he has told only half of the story of the exchange process. Yet half of the remaining story involves the government agencies with which the Special Division fought, and Corbett’s approach tells us little of how those agencies operated. As a result, we see not one-half but only one-quarter of the story.

For example, Corbett clearly demonstrates that intelligence divisions within the Army and Navy departments as well as the FBI blocked the exchange of certain Japanese nationals whom the intelligence agencies believed knew too much. But we learn only what the Special Division thought was going on in those agencies. A more widely based research effort employing Navy, War, and Justice department records would permit a more fully developed story. Similarly, when the officers of the Special Division clashed with officials of the War Relocation Authority over reuniting families or treatment of interned Japanese and Japanese-Americans, Corbett tells the story only from Special Division files.

What is left is a scholarly monograph on a very narrow subject about which little has been published. It makes a contribution to our knowledge of World War II both by explaining what the Special Division did and by showing that there were men who worked to preserve the civilized rules of warfare in a war fought without mercy.

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THEODORE COHEN. *Remaking Japan: The American Occupation as New Deal*. Edited by HERBERT PASSIN. New York: Free Press of Macmillan. 1987. Pp. xxiii, 533. \$27.50.

Publication of this book is a major event in the field of Japanese studies. Its insider description of issues, procedures, and personalities makes it the

most revealing story of the occupation yet to appear. Theodore Cohen was a brilliant and idiosyncratic member of General Douglas MacArthur's civilian staff. (Civilians, of course, did most of the substantive work of the occupation, while the military provided housekeeping services and security.) As a youngster of twenty-seven, he was appointed chief of the Labor Division at General Headquarters (GHQ) and was, therefore, MacArthur's most visible and vulnerable senior official. After sixteen months, Cohen was promoted to the position of economic adviser and special assistant to General William Marquat, chief of the Economic and Scientific Section. Following five years of service in the occupation, Cohen spent twenty-three years in Tokyo as a businessman. That experience deeply enriched his knowledge of Japan and his reflections on the occupation itself. This memoir, however, is based not only on Cohen's prodigious memory but also on extensive research, interviews with key officials of the occupation era in Washington and Japan, and his own voluminous archives.

The book is a distinctly revisionist account, especially of the role of MacArthur. Cohen begins with the widely accepted belief that the general was his own boss during the occupation, deciding the formulation and execution of policy as he liked. Policy, however, including the democratic reforms for which he received so much acclaim, did not originate with him at all. Policies were prescribed in a secret directive from the Joint Chiefs of Staff, which MacArthur received from Washington at the outset of the occupation. Unknown to the public at large or even to his own headquarters, this directive specified that, as his primary mission, MacArthur was to impose democracy on Japan. It ordered him to "encourage and show favor to policies which permit a wide distribution of income and ownership of the means of production and trade" (p. 12). It also ordered, among other reforms, the breakup of nationalist organizations, freedom of speech and assembly, removal from office and disqualification of "undesirables," economic and labor democratization, and educational reform. Cohen explains that MacArthur agreed with these radical reforms because his ideology was "essentially populist" (p. 457). That people mistakenly "thought he was ultra-conservative" (p. 457) helped their implementation. The only important policy (apart from women's suffrage) not included in the directive but adopted by MacArthur on his own initiative was land reform. This, however, was probably the occupation's most successful program. Together with the changes prescribed in the secret directive, it helped generate a mass consumer society in Japan and was essential to the achievement of the

economic "miracle" of Japan's first two postoccupation decades.

Cohen emphasizes that, although the general "did not know it" (p. 457), the reforms he was ordered to carry out were inspired by New Deal ideas. "The result was the transformation of a society based on mass sacrifice of the vast majority for the state to one based on democratic concepts of equity and justice" (p. 457). I would rephrase that sentence to say that the reforms made some adjustments in Japan's institutional arrangements, resulting in a transformation of the structure of economic incentives. Was Japan "remade"? At the end of World War II, Japan was ripe for communism, and it is to MacArthur's credit that he saved it from that fate. By the same token, it was more than ready for New Deal reforms. The United States simply helped Japan, I believe, to realize its own potential, which without the occupation would have remained dormant longer.

Among other misperceptions, MacArthur is usually credited with having prevented the Russians from sharing in the occupation of Japan. On the contrary, according to Cohen, "MacArthur was in accord with the principle of Soviet participation" (p. 60). The Russians, however, deliberately passed up an opportunity for military occupation. "Nothing MacArthur himself did averted the arrival of Russian troops" (p. 61). But Cohen's account should not be construed as diminishing his admiration for the general, whose "monumental role" and "enormous contribution" Cohen deeply respected (p. 459).

Among the most striking of Cohen's revisions is his assertion that MacArthur lost control of the occupation in early 1949, when Joseph Dodge, a Detroit banker, arrived in Tokyo. As a personal representative of President Harry Truman, he had authority to take charge of the Japanese economy. In Cohen's account, this development stripped MacArthur of most of his responsibilities, for by that time curbing inflation and controlling the money supply were superordinate to all other objectives of the occupation.

In his analysis Cohen also identifies key Washington New Dealers who planned policy and key New Dealers in Tokyo who helped implement it. He provides juicy details of bureaucratic politics in both camps and describes at length the infighting within the "Byzantine" GHQ. Nuggets of insight and information abound. For example, it was "one of the best-kept secrets" of the occupation that MacArthur sabotaged efforts to extract industrial plant reparations from Japan (p. 153). And the three-step formula devised to aggrandize MacArthur's authority in Japan at the expense of the other Allies was as follows: "First, grant the Allies participation in 'policy' (through the Far Eastern

Commission); second, reserve all 'operational' matters to the Supreme Commander, an American; and third, consider everything important to be operational" (p. 56).

Sadly, Theodore Cohen died before seeing his manuscript in print. In life, as in this book, he taught me and many others much about Japan.

LEON HOLLERMAN
St. Olaf College

WILLIAM STIVERS. *America's Confrontation with Revolutionary Change in the Middle East, 1948-83*. New York: St. Martin's. 1986. Pp. ix, 132. \$24.95.

William Stivers has set himself a daunting task in this slight volume. In barely over one hundred pages he attempts to demonstrate how the United States since World War II has consistently sought to repress radical nationalism in the Middle East. Economic and strategic concerns have dictated that course. The need to secure Europe's and later Japan's access to Middle Eastern oil and to contain the Soviet threat have led policy makers from the administration of Harry Truman to that of Ronald Reagan to secure American interests by propping up an unpopular conservative status quo.

Stivers sees two problems, support for Israel and the declining British colonial system, that have complicated the formation of an effective American policy. No administration has been able to guarantee Israel's survival while promoting a pro-Western stability among the chronically unstable Arab states. And, as the British military presence steadily eroded, the United States sought an alternative to a direct American role. A budget-strapped Richard Nixon and his administration saw American-armed Iranian and Saudi Arabian surrogates as an affordable answer. Massive transfers of high-tech arms allowed the shah, and to a lesser degree the Saudis, to maintain a pro-Western regional order.

Jimmy Carter's administration largely accepted Nixon's formulation before falling afoul of the revolution against the shah. And Stivers criticizes the Egyptian-Israeli accords as a narrow substitute for a more comprehensive Middle Eastern settlement. Carter, he observes, simply lost interest in the Palestinians and turned his back on the intractable problem of autonomy. Like his successor and Reagan's advisers, Carter came to see military power (the Rapid Deployment Force) rather than political or economic initiatives as a solution. The Reagan administration more simply concluded that, no matter what the local realities, a more aggressive American military posture would create a new respect that would obviate once insuperable problems.

For those who accept Stivers's critical views of American Middle Eastern policy, this book offers little that is new or startling. Every major event from the overthrow of Mohammed Mossadeq in 1953 to the invasion of Lebanon in 1982 has been covered in greater depth elsewhere. And the book suffers from the narrow Washington window through which it views the varieties of Middle Eastern nationalism. They exist only as a monolithic problem with which policy makers must contend. In that light it is hard to know from Stivers's analysis what form a constructive American policy might take. And those hostile to his formulation will find the coverage too broad to be either informative or persuasive.

But on one topic the book does make an important contribution. Stivers traces the role of naval strategists who began in 1959 to advocate the establishment of bases on the islands of the Indian Ocean. As colonialism retreated and Third World leaders grew less tractable, an island such as Diego Garcia offered a way for the navy to assert American power on a global scale and especially in the Persian Gulf. What began as a center for naval communications became by the Reagan administration a key base for those eager to revive gunboat diplomacy. An Indian Ocean military strategy for the Middle East, outmoded even as it was devised almost thirty years before, substituted for a comprehensive political strategy. By placing this little-regarded and seldom-debated policy center stage, Stivers justifies this book no matter how slight it might otherwise be.

MARK H. LYTLE
Bard College

JAMES CHACE AND CALEB CARR. *America Invulnerable: The Quest for Absolute Security from 1812 to Star Wars*. New York: Summit of Simon and Schuster. 1988. Pp. 367. \$19.95.

Redefinition, clarification, and synthesis are the key words that characterize this important study of the evolution of security policy throughout American history. James Chace and Caleb Carr contend, accurately, that there has been a fundamental flaw in American foreign policy from the revolution through Star Wars; although some formulators of policy have been more perceptive of the limits of power than others, this flaw has been the consistent pursuit not merely of security but of absolute security. The authors accept the realists' appraisal that policy ends and means must be balanced and that too often this equilibrium has not prevailed in the goals of the formulators of American foreign policy. The authors also contend that isolation was seldom part of the percep-

tion of policy makers; they argue that absolute security "cannot be negotiated; it can only be won"; therefore, the United States sought not isolation, but solitude. This is another way of saying that isolationism was merely a part of the happy circumstance that early on helped keep America from direct involvement in European politics except when American leaders chose to be involved in quest of total protective barriers to unwanted interference in American affairs (p. 13). Fear of that interjection of foreign influence and alien ideas has created a siege mentality among American leaders that has led them to suspect all foreign contacts not initiated by themselves.

Although the authors present overpowering evidence for their contention that real diplomacy was too often rejected in the name of aggressive defense of American aims, they do stretch the point to prove their thesis. There were times when practitioners of the art of diplomacy existed among American leaders, albeit, mostly in the nineteenth century, but Chace and Carr seldom give these diplomats credit for avoiding conflicts. Strangely, when they speak of James K. Polk's efforts to ensure absolute security via expansionism, they barely mention the search for ports in the Oregon country and the attendant defensive advantages sought in those environs, and they do not cite Norman Graebner's *Empire on the Pacific* (1955) at all, though some reference to his arguments would have greatly reinforced their contentions concerning Polk's search for absolute security. Several times in the book they ignore efforts to eliminate external competition by negotiation and to avoid war by the same process; for example, Theodore Roosevelt's aims at the Algeiras Conference are not noted. And, concerning his role in the Panamanian revolution, they give him more credit for initiation than he deserves; they assert that it was an "American-inspired" revolt (p. 138) when American-approved is more accurate. When discussing the diplomatic objectives of the Washington Conference of 1921–22, they fail to cite the definitive work on that topic, Thomas Buckley's *The United States and the Washington Conference, 1921–1922* (1970), and, in assessing Republican policy in the 1920s, they do not refer to the most startling new interpretation of the consistency of Wilsonian diplomacy into the Republican era, Peter Buckingham's *International Normalcy: The Open Door Peace with the Former Central Powers, 1921–29* (1983).

Despite these omissions, Chace and Carr present a cogent case for their central thesis and are especially sharp in assessing the underlying causes for the cold war and for the problems with Central and South America after World War II. They place the blame for the cold war on both

American and Soviet leaders but emphasize that the United States had the opportunity to ameliorate the strained relationship and once again sought an absolute rather than a realistic security, refusing to recognize legitimate or irreversible Soviet interests. Intransigent unilateralism has characterized American policy, reaching a crescendo in the Ronald Reagan years when Reagan attempted to restore both American prestige and Pax Americana with half-baked schemes based on a perception of the availability of superiority in the struggle with the Soviets, which assumed technological breakthroughs in the Strategic Defense Initiative that were chimerical.

The authors conclude that there have been some improvements in the American and Soviet views of the external world but that there can never be a truly effective relationship until American and Soviet leaders realize that security can never be more than relative, and this is merely to "admit a condition with which our country has always lived" but has "seldom been willing to acknowledge" (p. 20).

EDWARD M. BENNETT
Washington State University

CYNTHIA HARRISON. *On Account of Sex: The Politics of Women's Issues, 1945–1968*. Berkeley and Los Angeles: University of California Press. 1988. Pp. xxii, 337. \$25.00.

With this path-breaking account of women's issues on the federal level, Cynthia Harrison joins other historians such as Leila Rupp, Verta Taylor, and Sherna Berger Gluck who have reoriented historical attention toward the 1940s, 1950s, and early 1960s for the roots of the modern women's movement. Although the general outlines of the story are familiar, Harrison has given us far greater detail and analysis of the policies and players on the federal level from 1945 to 1963 than any other historian to date. The result is a readable and stimulating view of a critical period in twentieth-century women's history.

Harrison introduces the organizing concept of the "Women's Bureau coalition" (p. 8) to describe the various individuals, federal agencies, and national women's organizations that worked together to further women's issues in the postwar period. Harrison documents how many of the policy breakthroughs achieved during the early 1960s—such as equal pay legislation and a commission on the status of women—had been on the agenda of the Women's Bureau coalition since the 1940s. She demonstrates how these initiatives were implemented before any widespread popular demand for change in women's status.

Given the recent historiography on John Kennedy's presidency, the generally strong marks Harrison gives to the Kennedy administration on women's issues stand out as one of the book's most interesting contributions. Separating Kennedy's personal treatment of women as sex objects from his political awareness of women's changing social and economic roles, she details how the New Frontier expanded options for women, especially as it moved beyond the older strategy of recognizing women through appointments of prominent women to support for policy initiatives that addressed women's social and economic position more directly.

Harrison offers a full and cogent analysis of the strengths and limitations of the Presidential Commission on the Status of Women, 1961–63; her discussion of Esther Peterson's role in breaking the stalemate over the Equal Rights Amendment is especially interesting. Although Harrison applauds the commission's commitment to expanding opportunities for women in the public realm, she also points out how often its recommendations affirmed women's traditional roles. Without claiming that the Presidential Commission on the Status of Women caused the revival of feminism, she provides evidence that the commission did make a difference, especially in raising expectations and establishing networks of interested activists. Here her interpretation follows that set forth by Jo Freeman in *The Politics of Women's Liberation* (1975).

Two turning points between World War II and the beginnings of feminism as a mass movement in the late 1960s stand out in this account. The first occurred between 1961 and 1963 when the federal government for the first time in the postsuffrage era enunciated a federal policy on women's issues. The second took place in 1966 with the founding of the National Organization for Women (NOW), which shifted the momentum on women's issues from women and men linked to the federal government to an outside group that could act as a catalyst for broader change. NOW was significant not only because it was independent but also because it moved beyond the presidential commission's belief that sex roles were immutable to an emphasis on women's equal access to the public realm. And, showing how quickly things were happening in the 1960s, NOW soon endorsed the Equal Rights Amendment, the very legislation that the Presidential Commission on the Status of Women had been set up to forestall.

This study provides a judicious and comprehensive survey of public policy on women's issues at the national level over a critical twenty-three-year period. More than anything else, the book describes the strengths and limitations of the liberal

feminist philosophy that meaningful social change can occur without radically altering the political and economic structure. The accomplishments of the members of the Women's Bureau coalition—activists whom Betty Friedan once called the midwives of the modern women's movement—suggest the utility of Harrison's concluding image: "If you want to get a camel into someone's tent, ask if you can just put its nose under the tentflap" (p. 221).

SUSAN WARE
New York University

JAMES L. BAUGHMAN. *Television's Guardians: The FCC and the Politics of Programming, 1958–1967*. Knoxville: University of Tennessee Press. 1985. Pp. xvi, 311. \$27.50.

This book on the Federal Communications Commission (FCC) might appropriately be described as a neorevisionist view of broadcast regulation in the 1960s. The revisionist interpretation of federal regulatory agencies asserts that these bodies become captives of the industries that they are supposed to regulate and end up serving private, rather than the public, interest. But James L. Baughman's thesis is that, although such an interpretation fits the FCC's behavior until the late 1950s, after that a different dynamic was in operation.

With the election of John F. Kennedy, the makeup of the FCC changed dramatically. The political folklore of the Kennedy-Johnson years claims that idealistic technocrats staffed the federal regulatory agencies to do battle with vested corporate interests, but in many cases the power of the corporations was too great to overcome. Although the broadcasting industry did resist change and oppose strong regulation in the 1960s, Baughman argues that the reason for the FCC's failure to create any revolutionary change in television lies elsewhere.

The author successfully re-creates from an exhaustive mining of primary documents and interviews with many key participants what actually happened to frustrate the goals of the FCC in the Kennedy-Johnson years. At first glance it would appear that the agency in that period was programmed for success. The reformer Newton Minow was appointed FCC chairman and eventually was provided with a majority of like-minded commissioners. Minow's rhetoric (for example, television programming as a "vast wasteland"), however, was never matched by the regulatory performance of his agency. From Baughman's analysis there appear to be two major reasons for this.

First, the vision of quality television program-

ming held by Minow and other critics was not shared by the American public at large. When Minow took his pleas for changing television through FCC action directly to the television audience in the form of public hearings in different parts of the country, he was met with criticism and even contempt. Part of this response was no doubt industry orchestrated, but part of it reflected the elitist taste in programming that Minow and many intellectuals who criticized television held. Rather than finding his proposals welcomed with open arms by the viewing public, Minow found that much of the public apparently liked the "vast wasteland" of commercial television.

Second, and more important, Minow's plans for reforming television often clashed with the political interests of key members of Congress and of the president. Minow's vision for television was national in scope, but members of Congress were sensitive to the demands of local constituencies. When the goal of quality programming came into conflict with provincial tastes or the commercial interests of the people "back home," members of Congress invariably deferred to their constituents. Furthermore, while President Kennedy may have shared some of Minow's views on television programming, he was most interested (as was President Johnson) in his own political career. If FCC policy was seen as undermining political support, or interfering with the relationship every president attempts to cultivate with the broadcast media, then the FCC was abandoned by the president.

The result of this political situation was that the reform-minded FCC was continually frustrated in its attempt to create meaningful change in commercial television broadcasting. Baughman's documentation of how well-meaning regulators in a relatively liberal political climate ended up with a performance record not much different from that of venal, incompetent, and often dishonest regulators in earlier periods paints a convincing picture of the limits to the regulatory process. This book is an important contribution to the history of federal regulation and to the understanding of the powerful nexus between politics and broadcasting in America.

STEWART LONG
California State University,
Fullerton

CANADA

ANGUS McLAREN and ARLENE TIGAR McLAREN. *The Bedroom and the State: The Changing Practices and Politics of Contraception and Abortion in Canada, 1880-1980*. (The Canadian Social History Series.)

Toronto: McClelland and Stewart. 1986. Pp. 186. \$14.95.

This short study rehearses and assesses key aspects of the twentieth-century public debate over the extent to which Canadian women should have open, legal access to birth control knowledge, methods, and technologies. Although the book contains demographic information, it is not a demographic study. Instead, Angus and Arlene McLaren are interested in tracing what might be called the publicizing and politicizing of birth control in Canada.

The task is a difficult one, partly as a result of the dramatic ethnic, religious, and demographic diversity of Canada and partly because the authors found no figure comparable to Margaret Sanger in the United States or Marie Stopes in the United Kingdom around whom they could organize their material. Moreover, the time span suggested in the subtitle is misleading. The McLarens make a few references to the nineteenth century and append a superficial discussion of the national legislation of the late 1960s and early 1970s, but they devote most of their attention to the era between 1920 and 1960.

Open discussion and public advocacy of birth control in Canada began, in this analysis, among certain members of the feminist Left in British Columbia during the 1920s, although neither the Canadian Left as a whole nor Canadian feminists as a whole endorsed birth control at that time. The movement spread eastward during the 1930s, when A. R. Kaufman of Ontario, operating from a mix of eugenicist and middle-class motives, expanded both the popular awareness and the public distribution of birth control ideas and devices in central and eastern Canada. Only after the Second World War did the birth rate in Quebec plummet to the levels already characteristic of the rest of Canada, thereby making politically possible national legislation that overtly decriminalized birth control activities and opened the door to legal abortions.

Although this book has the quality of separate essays on aspects of a general subject, as distinguished from an integrated account, many of the essays contain excellent material. A sharp reassessment of maternal death rates in British Columbia from 1922 to 1968, for example, allows the authors to make with special force their point that governmental decisions to withhold certain types of reproductive information and services (abortion in this case) had quite literally life-and-death consequences. Another essay explores in the Canadian context a subject that Angus McLaren has previously written about in the context of both England and France: the relationship between

socialism and Malthusianism. The McLarens' account of the famous Palmer Trial (1936–37) and their analysis of the way in which Kaufman pressured the Canadian medical establishment to shift its attitudes toward birth control are especially well done. Citations throughout reveal thorough research in the original sources.

Some scholars will disagree with a number of the McLarens' assumptions, and their book contains several examples of special pleading. Nonetheless, all historians interested in modern population questions will welcome this important and well-grounded contribution to the literature. A sustained, accessible discussion of the Canadian situation has been sadly lacking. Teachers of Canadian social history should also find this paperback edition useful in class.

JAMES C. MOHR
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SUZANNE ZELLER. *Inventing Canada: Early Victorian Science and the Idea of a Transcontinental Nation*. Toronto: University of Toronto Press. 1987. Pp. vii, 356.

The practitioners of early Victorian Canadian science, in an attempt to garner support and as an expression of their sincere hopes for their activities, tended to overestimate the significance of their work for political, economic, and cultural development. William Logan, founding director of the Geological Survey of Canada in 1841 and Canada's most successful Victorian scientific entrepreneur, "always managed to find some new promise of untold mineral wealth to maintain public interest in his endeavours" (p. 56). J. H. Lefroy, who managed to build the Toronto Magnetic and Meteorological Observatory into a major institution, sold himself and the public on the importance of his work by emphasizing the dubious relation between magnetic effects and epidemiology (p. 139) as well as the potentially great benefits of climatological information for encouraging immigration (p. 155). And George Lawson, founder of the Botanical Society of Canada, drummed up support for the society's activities by focusing on the application of native plants to agricultural and industrial uses (p. 234). In each of these cases, the initial promises were only partially fulfilled, and they ultimately became subordinated to patterns of very substantial, if not so popularly spectacular, scientific research.

The relationship between what Suzanne Zeller promises in her title and what she delivers in her text is surprisingly like the relationship between the promises and the reality offered by most of the

founders of those Canadian "inventory sciences"—geology, meteorology, terrestrial magnetism, and botany—whose history she recounts from approximately 1820 to 1870. In her title, the author suggests that early Victorian Canadian science is interesting primarily because it stimulated Canadian political consciousness and encouraged a vision of national identity that encompassed a region stretching from the Atlantic to the Pacific. The ideological implications of science is a very hot topic at the moment, so the promise of the title should draw attention to the work. Furthermore, the author not only seems to believe what her title suggests but even manages to show that geological and meteorological studies tended to discount political boundaries and to encourage the expansion of geopolitical units of analysis, and she clearly shows that such expansive nationalist movements as the Clear Grit movement of the 1850s and the Canada First movement, begun in the early 1860s, drew much of their rhetorical imagery from studies of plant distribution and variation, describing Canadians as variations from an initial British stock created in response to their New World environment. But explicit discussions of the relationship between science and ideology take up only about 15 percent of the space in this book, and they are far less clear, detailed, and carefully done than I had hoped.

The vast bulk of this work—and to my mind the most valuable portion—constitutes a careful study of the cultural context of nineteenth-century Canadian science and of the way in which the inventory sciences grew to maturity within that context. It draws from an impressive breadth of manuscript materials, contemporary periodicals (including local newspapers as well as scientific journals), and recent secondary accounts. And it offers rich insights into the politics, the personnel, and the institutions of Canadian science and science education.

For a work that deals so extensively with exploration and the geographical distribution of minerals, plants, and climatic conditions to be without any maps to help orient the reader is a real shame.

RICHARD OLSON
Harvey Mudd College

DONALD B. SMITH. *Sacred Feathers: The Reverend Peter Jones (Kahkewaquonaby) and the Mississauga Indians*. (American Indian Lives.) Lincoln: University of Nebraska Press. 1987. Pp. xix, 372. \$22.95.

Donald B. Smith has made another valuable contribution to the study of Canadian Indian history. His subject, the life of the Reverend Peter Jones, is presented with the care for detail and context that

characterized Smith's earlier biography of Buffalo Child Long Lance.

Peter Jones's career touched on many important aspects of Indian history in Canada before Confederation. The son of a Welsh surveyor and an Ojibwa mother, Jones was reared with his mother's people. In his teenage years he attended school and came under the tutelage of his father. Converted to Methodism, Jones became a missionary, pastor, teacher, fund raiser, public speaker, international traveler, and chief of his band. He was involved in the Methodist disputes about American and British affiliation and was an associate of George Copway, the Ojibwa Methodist cleric who defended Indian removal in the United States. Jones was the most outstanding of a cluster of Methodist Ojibwa clergy in the mid-nineteenth century in Upper Canada. Another was Henry Bird Steinhauer, missionary to the Cree of western Canada.

Jones's fund raising in England acquainted him through Exeter Hall with many of the leading Evangelicals, especially among the Dissenters. He had the customary interview with the monarch and met the aging Hannah More and Thomas Hodgkins. In England, Jones met and married a wealthy Englishwoman, and in Canada and in the United States they experienced the venom and hostility of race prejudice.

Jones lived in a time when the Indians had been pushed aside by the influx of white settlers. The fur trade partnership and the wartime alliances were past for eastern Canada. Much of Jones's public life was concerned with assisting the Ojibwa in cultural change in order to avoid extinction. Jones himself became a target of church and government schemes directed at Indians generally. To survive, Jones thought, the native peoples must submit to considerable change and adaptation. Jones chose to accommodate and urged other Indians to do the same. His career was set well before the Indian rebirth of the middle and late twentieth century. The marginalizing of Indians extended to Christian Indians. Continued paternalism led to an erosion of Methodist Ojibwa enthusiasm in the late nineteenth century.

Smith has provided extensive information on the historical context of the Ojibwa settlement in southern Ontario. Jones's role as an Indian activist allows the author to discuss early Indian protest organizations and the issues that concerned them. Despite his willingness to accommodate, Jones wished to preserve the Indian people and their communities. He found much that was valuable and meaningful in native life. He rejected the notion of assimilation, which was government policy from the 1830s, and worked for the creation of Indian communities that were Christian, settled,

agricultural, and selectively adaptive. He sought to have change with continuity. Indian communities, he thought, should be self-governing and hold a secure land base. To accomplish this he believed it might be necessary to consolidate some communities. Jones provided leadership in a difficult period. His sermons illustrated the blending of European and Ojibwa that he envisaged. To help preserve native culture, he wrote and spoke on Ojibwa history and traditions. In his view Indian culture was not static but adaptive and dynamic.

Smith's splendid study fully explores the impact of the newcomers on Jones and his people. This is an important piece of scholarship in Indian biography and in Canadian Indian history generally.

E. PALMER PATTERSON
University of Waterloo

A. M. J. HYATT. *General Sir Arthur Currie: A Military Biography*. (Canadian War Museum Historical Publications, number 22.) Buffalo, N.Y.: University of Toronto Press. 1987. Pp. xii, 178. \$24.95.

The First World War had a major impact on Canada. With a population of 7.5 million in 1914, the country raised an expeditionary force of over six hundred thousand men, most of them volunteers. It had only a tiny regular army, and among both the regular and reserve forces there were few officers who had either fighting experience or had even attended staff college. Command of the first Canadian division to arrive overseas was given to an experienced British general. By the end of the war, Canadians filled most of the senior posts, and the Canadian Corps, composed of four divisions, was led by Lieutenant-General Sir Arthur Currie.

Currie began his military career as a young gunner in the Fifth Regiment, Canadian Garrison Artillery, in Victoria, British Columbia. In this militia unit he achieved steady promotion until in 1909, at the age of 34, he commanded it. Three years later he became the commanding officer of a local Highland regiment, a position he could not afford owing to his failing business practice, which also resulted in financial embarrassment during the war. His military efficiency, though limited to a militia battalion, was such that in 1914 he was given command of a brigade. In part this may have been because of his friendship with Garnet Hughes, son of Canada's erratic but vigorous minister of militia. The minister's demand that his son should command a division in action plagued Currie throughout the war.

A. M. J. Hyatt's biography is both a description of Currie's rise in command from a brigade to a corps commander and an account of Canadians fighting on the western front. In each instance it

was a case of learning "on the job" in a conflict fought quite differently from any former war. Currie, a large, reserved man with all the charisma of a bayonet, learned the lessons of trench warfare quickly. At the Somme, for example, he made it his business to find out why things went wrong. He interviewed officers and men, questioned the reasons for high casualties, and sought ways to deal with the hard facts of barbed wire, machine guns, and trenches. He insisted that the lower ranks be informed about the battles they were to enter and, when the opportunity offered, that the units be rehearsed for the attacks they were to make.

Currie's attention to organization and cooperation won battles and saved lives. Trench warfare did not permit him the battlefield maneuverability that in a later war captured headlines for an Erwin Rommel or a George Patton. But, within the confines of trench warfare on the western front, Currie and his Canadian Corps gained a reputation that was second to none among the Allied formations. According to Hyatt, Currie "created in the Canadian Corps an admirably efficient military instrument" that, in the last months of severe fighting, "liberated 228 cities, towns and villages," "confronting and overcoming in the process forty-seven German divisions" (p. 118). The amateurs had become professionals, and Currie himself gained a respect among the veterans that probably pleased him more than the honors he received at Buckingham Palace.

Hyatt's well-written book is a critical yet balanced account of an outstanding Canadian soldier whose achievements deserve greater recognition. It is a "must" for any military library.

R. H. ROY
University of Victoria

DAVID R. ELLIOTT and IRIS MILLER. *Bible Bill: A Biography of William Aberhart*. Edmonton, Alberta: Reidmore. 1987. Pp. ix, 372.

William Aberhart was judged by Roger Graham ("Charisma and Canadian Politics," in John S. Moir, editor, *Character and Circumstance: Essays in Honour of Donald Grant Creighton* [1970], p. 35) as the "most certifiable exemplar" of a charismatic Canadian in the last century. David R. Elliott and Iris Miller substantiate Graham's judgment. Building on several previous studies of Aberhart and the Social Credit party, Elliott and Miller go beyond these works to make use of the Aberhart papers, both private and official, and church records not previously used. Their new perspective is that Aberhart's political activities, as premier of Alberta from 1935 to his death in 1942, were not an extension of his religious sectarianism; rather,

"the Great Depression forced Aberhart out of his other-worldly approach to life, derived from dispensational pre-millennialism" (p. 318). The second element of their thesis is that Aberhart was not right-wing in his political leanings but left-wing, a charismatic, left-wing fascist (p. 320).

Born in 1878, Aberhart grew up in a Canada in which religion was the single most important formative element. Society was faced with the fundamentalist versus modernist controversy. Aberhart came to adulthood adopting his own form of dispensationalism, with emphasis on the afterlife. A dichotomy between his beliefs and his actions appeared in his choice of education as a vocation with its emphasis on helping young people cope with the present in order to shape the rest of their lives. Aberhart's ultradispensationalism came to be molded by his personality into a highly personal theology. He preached this theology in Calgary when he founded the Calgary Prophetic Bible Institute, an offshoot of his own church. His life then, as later on, was characterized by a need to be a founder, not a follower, and to react strongly against anyone who challenged his authority. His style of leadership was based on his ability to manipulate people. He thus generated strong reactions. The greatest tragedy of his life was that his drive and ideas alienated many of those he tried to help, costing him achievement of his strongest desire, which was the gratitude of the people he wanted to help.

The first instrument of his leadership was the institute through which he educated his followers in theology, but more important, in acceptance of his view of life. Probably the single most important instrument he used was the radio. Projecting his personality through the microphone, he was able to rise above his competitors in the field of religious leadership, educate a much larger sector of the public, and widen his financial base. He thus constructed the base from which he later made his meteoric rise in politics as leader of the Alberta Social Credit party. The third factor in his success was his conviction that he was the instrument of Providence for accomplishing good.

The depression of the 1930s generated the circumstances that forced Aberhart beyond the field of religion. Seeking some means of alleviating the destitution in Alberta, he came in contact with the theories on social credit of C. H. Douglas. Attractively simplistic concepts expounded by the charismatic Aberhart and an intensely loyal following provided the ingredients for political success: fifty-six of sixty-three seats in the Alberta legislature went to the Social Credit party in its first bid for office.

From this political base, Aberhart was able, in his six and a half years as premier, to accomplish

much in health care, labor, education, oil and gas conservation, and moratoriums on mortgage foreclosures. All of these issues were unrelated to Social Credit ideology. In the field of monetary reform, Aberhart met failure. His need as premier to function within the realities of power and responsibility to the electorate led to irreconcilable differences with Douglas, the theoretician. Receiving no help from Douglas, Aberhart tried numerous ways of his own to implement the concept of the social dividend. Here he was stymied by the courts and the federal government.

Bible Bill should stand as the definitive study of Aberhart for some time.

WELF H. HEICK
Wilfrid Laurier University

LATIN AMERICA

RALPH H. VIGIL. *Alonso de Zorita: Royal Judge and Christian Humanist, 1512–1585*. Norman: University of Oklahoma Press. Pp. xiii, 382. \$28.50.

Biography has become an important genre again in Latin American history. This contribution by Ralph H. Vigil seems destined to cause a serious reevaluation of the life and career of Alonso de Zorita. In this exciting study, Vigil carries the reader into the intricacies of life and political administration in the sixteenth-century Hispanic world. This book not only chronicles the life of an important jurist and writer but also attempts to re-create the character of life in Spain and its colonies during the time of Zorita. The study is solidly grounded on archival and extensive bibliographic research.

The book is divided into seven chapters that cover the important phases of Zorita's life. We see the jurist as a young man in late fourteenth-century Spain and his professional development at the University of Salamanca. From there he served as a justice on the audiencia of Santo Domingo. Growing out of his judicial appointment came a stint as *visitador* to New Granada charged with the enforcement of the New Laws. After that he served first on the audiencia of Guatemala, then that of New Spain during the time of the abortive Cortés conspiracy. Zorita finally retired to Spain, where he wrote his justly famous historical-anthropological works. Vigil's study of this important jurist and thinker's life is a significant contribution to scholarship about the Spanish humanitarian movement after Bartolomé de Las Casas.

There are, nevertheless, several disquieting features of the work. Perhaps the most unnerving is what seems to be Vigil's uncritical acceptance of primary sources without analyzing the context or

material. This tacit acceptance of primary sources sometimes leads to misrepresentation of the facts. In discussing the family of Hernán Pérez de Bocanegra, Vigil has Pérez's sons marrying nieces of the viceroy, Don Luis de Velasco, the Elder. Vigil bases this account on reports sent by the *visitador* Lic. Jerónimo Valderrama to the Council of the Indies. But many "relatives" reported by Valderrama were no kin to Velasco, including the Pérez Bocanegra family. Unfortunately, Vigil did not go beyond the document. This is curious, since he also commented on the enmity between Velasco and Valderrama (pp. 232–33), which should immediately have made him question whatever information Valderrama presented about Velasco. But, beyond this, Vigil confuses Pérez Bocanegra's mother-in-law, Doña Beatriz de Estrada, with the viceroy's sister-in-law, Doña Beatriz de Andrada (p. 201, n. 111), an error that is not attributable to Valderrama.

In another instance, in the chapter detailing Zorita's activities enforcing the New Laws in New Granada, Vigil repeats on numerous occasions the complaint that officials, encomenderos, and other Spaniards loosed dogs on the Indians and hunted them down (pp. 91, 107–09, 113–14). Yet the reader never knows how much of this was real and how much was merely included in the complaints for shock value. Many passages seem to be simply translations of complaints, questionnaires, and other official papers without much critical analysis. The picture described by Vigil is one-sided, with little attempt made to explain the actions of those who opposed Zorita or to cut through the verbiage of the complaints and charges to catch a glimpse of what really happened. Although his assessment of Zorita's contributions to Mesoamerican anthropology is quite fair, he saves until nearly the end any of Zorita's faults.

Another feature of the work that causes uncertainty in the reader is Vigil's failure to recognize the honorific *don* as a significant feature. Early on, talking about the proper form of Zorita's name, he cites scholarly criticism of those who used the honorific with Zorita (p. 10). Vigil errs in the other direction, removing the honorific, on occasion, from those who warranted it, leading to some confusion. In this period, those who deserved it always used it; those who did not, did not, with very few exceptions.

These aspects of Vigil's work tend to call into question his methodology and his interpretation of the documents. The study addresses many important questions and seeks to revise our view of events in early Latin America. Yet, if one cannot rely on the facts presented in the work, the interpretations must be called into question.

Vigil has presented a good summary of the

literature and research on Zorita and his era. This is an important contribution but one that must be read with tremendous caution and critical analysis.

JOHN FREDERICK SCHWALLER
Florida Atlantic University

JAMES E. OFFICER. *Hispanic Arizona, 1536–1856*. Tucson: University of Arizona Press. 1987. Pp. xx, 462. \$45.00.

This, the first serious attempt to synthesize all that is known of the Hispanic period, is the most important work published on Arizona history since *Be It Enacted: The Creation of the Territory of Arizona* by Benjamin Sacks appeared in 1964. James E. Officer has consulted virtually all published material, and his thirty-year search for fragmented archives has brought together a decent body of original Spanish and Mexican records from which he has extracted all of the sociological and ethnographic data these documents can yield.

An introductory chapter is both a survey of the subject and an antidote for popular misconceptions. It treats topics such as architectural styles and the origin of the name Arizona. Subsequent chapters consider the Pimería Alta of New Spain, northern Sonora as part of the Republic of Mexico, and the Yanqui invasion of the 1850s. An epilogue ties up loose ends of family history, economic development, and Apache wars, bringing the story to roughly the 1860s.

Chapter 2, "Early Exploration and Settlement," passes lightly over the explorations in Arizona by conquistadores and the work of Franciscan missionaries among the Hopi Indians. The author uncritically accepts the old works by Herbert Bolton and others, but more is needed here. The rest of the chapter brings the Jesuits into the Pimería, followed by itinerant miners and the military. In chapter 3, "The Provincias Internas," Officer confines himself to local affairs in northern Sonora without addressing larger problems of frontier defense or explorations north of the Gila River in search of a link between New Mexico and California. "The Last Years of Spanish Rule," chapter 4, briefly describes the Sonoran frontier at a time of relative calm.

The Mexican part of the book is the longest, seven of fifteen chapters, and embraces the shortest span, 1821–48. Although the political power struggle during most of this time between José Cosme Urrea and Manuel María Gándara is familiar, Officer points to Gándara's appeasement of Indians as crucial. The author uses the extensive work by Jay J. Wagoner on Mexican land grants

with new political insights, and he analyzes extant demographic data better than previous writers while acknowledging the help of Henry F. Dobyns. He also relies on solid previous work by John Kessell, Kieran McCarty, and other historiographical pioneers.

Indian conflict occupies much of the text. Mutual antagonism between Hispanos and Apachean peoples is well documented. In a land bereft of good firearms and short of horses, the soldiery barely held its own. Frontier-bred military leaders like Colonel José María Elías and Captain Antonio Comadurán are the book's most conspicuous figures. But Officer also shows the extent to which Apache *mansos* ("tamed" Apaches) were absorbed into Arizona's population, and he shatters the legend of peaceful Piman Indian farmers. They would have given the Hispanos trouble enough without Apaches.

Especially valuable is Officer's attention to family ties in sparsely settled Sonora. An essay on the Elías family forms one appendix, and there are separate genealogical charts devoted to Elías-Ortiz kinship and to the Comadurán and Ramírez families. These help explain land ownership and politics.

Casual readers may find the endless succession of Indian raids and the unrelieved poverty of Hispanic frontier life too grim to enjoy. There are gaping holes, too, in the narrative fabric left by the destruction and scattering of archives. The overall effect of the book is to cast the shadow of a former time, as ephemeral as a desert wind. Maps by Donald Bufkin and well-chosen illustrations amplify the text, and Officer's judicious interpretation of scant sources is impressive. It is the best job we are likely to have for a long time.

ANDREW WALLACE
Northern Arizona University

STAFFORD POOLE. *Pedro Moya de Contreras: Catholic Reform and Royal Power in New Spain, 1571–1591*. Berkeley and Los Angeles: University of California Press. 1987. Pp. xii, 309. \$40.00.

Pedro Moya de Contreras filled various posts in late sixteenth-century New Spain. After acting as first inquisitor of Mexico, he became the first archbishop of the colony appointed from the ranks of the secular clergy, *visitador* (inspector general of official conduct), and captain-general and viceroy. On his return to Spain he was appointed president of the Royal and Supreme Council of the Indies and was granted the title of patriarch of the Indies before his death in 1591.

Born into a family of the lower nobility, Moya

went to the University of Salamanca and studied jurisprudence. He thus was one of many *letrados* (university graduates and legists) who acted as agents of the crown in creating the absolute, patrimonial monarchy of the sixteenth century. Favored by Juan de Ovando, who became president of the Council of the Indies in 1571, Moya began his career as one of the inquisitors of Córdoba. As inquisitor in Spain and Mexico, Moya confirms Julio Caro Baroja's observation that the inquisitor was first a man of laws before he was a theologian.

As cleric and royal agent in Mexico, Moya represented the best and worst characteristics of post-Tridentine priests and bureaucrats in the reign of Philip II. Although he presided over the consolidation of the Mexican church and the civil government, other royal agents had already curbed the independence of spirit shown by the religious orders and their effort to create a Christian utopia for the Indians in the world's final age.

Before Moya's arrival in the colony in 1571, the partial repeal of the New Laws of 1542-43 signaled a turning point in crown policy that culminated in the end of the pro-Indian reform movement. With the death of Viceroy Luis de Velasco in 1564, the movement led by Bartolomé de Las Casas was further weakened. After the death of Velasco, the Liberator of the Indians and the Dominican's Vicar, the royal commissioner Jerónimo Valderrama assumed charge of the Audiencia of Mexico and increased Indian tribute payments under the guise of reform. He also denigrated the viceroy, the friars, the royal judges, and anyone else who differed with him.

As Stafford Poole notes in this well-written study based on archival materials and previous studies by students of colonial Mexico, Moya de Contreras is known for what he did rather than for what he was. More a symbol than a human being, the subject of this study was an enigmatic figure who "represented and summed up the movements of his age: regalism, emerging criollismo, imperial bureaucratization, the Catholic Reformation, the decline of Renaissance humanism as a force in church and society, the emergence of a new kind of dedication, centralization and authoritarianism" (p. 214).

In presenting the history of a man in the context of his age, Poole has written an interesting and informative biography that sheds light on a major but unknown figure in Mexican history. Specialist and generalist will benefit from this work: when related to other works it tends to confirm the end of the crown's reform efforts in New Spain, the triumph of Creole "feudalism," and the mixed

blessings of consolidation in society, church, and government.

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SUSAN MIGDEN SOCOLOW. *The Bureaucrats of Buenos Aires, 1769-1810: Amor al Real Servicio*. Durham, N.C.: Duke University Press. 1987. Pp. xxi, 356. \$45.00.

This work joins the list of titles that, over the last two decades, has addressed the changes that Spanish America experienced during the second half of the eighteenth century. By focusing on the bureaucratic regimen that the Bourbon monarchy attempted to institute, Susan Migden Socolow builds solidly on previous studies of the civil service in Spanish America that have been carried out by Jacques A. Barbier, Mark A. Burkholder, D. S. Chandler, and Leon G. Campbell, among others. All of them demonstrate the difficulties in effecting fundamental changes in the habits of governance that had characterized the Indies for more than two centuries.

Buenos Aires formed the territorial hub of the viceroyalty of the Río de la Plata, one of the new administrative units created by the Bourbons. Its creation in 1776 represented not only a means to more efficient management of the Potosí silver trade but also a recognition that the South Atlantic portion of the Indies formed a vital trading area and was required for regional stability between Spain and Portugal. As a new administrative territory, the viceroyalty of the Río de la Plata offered the Bourbons a unique opportunity to implement their revised and much more professional attitude toward public service.

Socolow's study amply documents the failure of many of the Bourbons' objectives, partly as a result of local agents who resisted conformity to a professional view of the public servant and partly as a result of the crown, which lacked the means or the will to implement its own theories of government. She begins her survey of the Platine bureaucracy by looking at the men who filled the highest ranks, viceroys and intendants, whose abilities she characterizes as fair to middling. These men had to pay their own way to Buenos Aires and finance much of the setup of the viceregal court and household. Because they seldom had the necessary funds, bureaucratic independence was undermined by the client connections that linked these powerful officials to merchants who advanced them the needed capital. The efficacy with which viceroys and intendants governed continued to depend on their personalities and political savvy, much as had

been the case prior to the Bourbon regulations that were supposed to affect these positions in positive ways.

Predictably, the bureaucracy grew fastest during the initial years after the establishment of the viceroyalty, a time when new administrative agencies were being created. After the period from 1776 to approximately 1783, however, the bureaucracy hardly expanded. This meant that promotions were slow in coming; officeholders continued to purchase their positions, Bourbon plans to the contrary notwithstanding; and not until one of them died or retired could someone in an inferior post hope to move up the bureaucratic ladder. Socolow deals with the manner in which bureaucrats received their appointments, experienced frustration with their limited horizons, and maneuvered for meager salaries and infrequent retirement benefits.

One of the most informative sections deals with the marital and familial connections with which public officials, especially at the middle levels of the bureaucratic corps, sank roots into the local society of Buenos Aires. Once again, merchants of all ranks appear to have benefited from the calculations associated with their daughters' marriages. The Bourbons had nothing of the sort in mind, wishing instead to isolate the professional corps of officials from the evils of corruption and favoritism that such liaisons were likely to spawn.

This work combines quantitative material with narrative and analytical elements in a wholesome balance. The graphic representations on the subjects of salaries and cost of living are crisply presented and discussed. Socolow commendably includes standard deviation figures in her tabular material, which quantitative historians unfortunately do not employ enough. Occasionally, however, Socolow fails to use the standard deviation for what it was designed to do: for example, to begin answering questions regarding the extent of chance in the differences in the average ages at marriage of local women and of the bureaucrats. She writes that "there was an interesting correlation between the position of the husband within the bureaucracy and the age differential between husband and wife. In general, the higher the bureaucratic rank of the husband at the time of marriage, the greater the age gap between him and his spouse" (p. 207). The table presented indicates an extremely interesting trend in which the wives of the highest (and oldest) bureaucrats tended to be much younger than those of the lowest (and youngest) ones. But a simple *t*-test (admittedly not the optimal computation, but the best I could manage, since the age at marriage for each individual included in her table is not available) shows that "high-level" and "middle-level"

bureaucrats did not marry at significantly different ages. Moreover, certain of the standard deviations are so high that some further statistical analysis was warranted to determine if the trend evident in the mean ages was indeed more than merely suggestive.

This is a relatively minor issue, however, when we take into consideration the very complete picture Socolow gives us of the front line of royal government in what was arguably the most dynamic region of imperial South America.

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RAMON EDUARDO RUIZ. *The People of Sonora and Yankee Capitalists*. (Profmx Monograph Series.) Tucson: University of Arizona Press. 1988. Pp. x, 326. \$35.00.

In the past dozen years Ramón Eduardo Ruiz has published three books on the Mexican revolution: *Labor and the Ambivalent Revolutionaries: Mexico, 1911–1923* (1976), *The Great Rebellion: Mexico, 1905–1924* (1980), and the volume under review. The first and last are monographs based on archival research. *The Great Rebellion* is a sweeping narrative meant for a popular audience. Together they assert that the Mexican revolution was not a revolution, for it did not produce fundamental change. It was started and won by mainly middle-class opportunists who possessed neither ideology nor ideals; its greatest leaders were either fools or frauds.

This study explores the origins of the victorious rebels, the northern middle-class Sonorans such as Alvaro Obregón, Adolfo de la Huerta, and Plutarco Elías Calles. Ruiz discovers that the Sonoran rebels adopted a capitalist, export-driven economic model, heavily dependent on the exploitation of peasants and workers and on the attraction of foreign investors, because this type of economy was the economic environment from which they had profited. They did not rebel against the economic system constructed by Porfirio Díaz to destroy it but, rather, to control it. They took up arms not because they had not benefited from it but because they had not benefited enough.

The author describes in some detail, enriched by quotes and anecdotes gleaned from Sonoran archives, the shortcomings of the state's export-oriented, foreign investment-based development during the Díaz era, 1880 to 1910. To Ruiz, the villains of the story are obvious: the Yankee capitalists, who took and took but never gave, and the Sonoran elites, who sold out their country.

Unfortunately, Ruiz does not differentiate between large (multinational) companies and more

modest businesses. Although employees of William C. Greene's Cananea complex may have remained isolated from and contemptuous of Mexicans, the smaller operators often learned Spanish, married natives, and frequently were victimized by local authorities (certainly in the 1880s and 1890s). There is no clear evidence, moreover, that foreigners mistreated their workers any more than did Mexican employers. The author does not account for change in the foreign presence over time. Nor does he explore thoroughly the relationships between foreigners and local elites.

Ruiz's sources, to some extent, limit his conclusions. Although he has deeply mined the archives of Sonora and used the U.S. consular despatches extensively, he has not cited other important sources of information on foreign companies, such as *The Engineering and Mining Journal* and other trade publications, the papers of the Cananea companies, and the published memoirs of numerous foreign entrepreneurs.

Most important, Ruiz has ignored the rest of Mexico. Comparing and contrasting Sonora's development with other states with heavy foreign investment, such as Chihuahua, or mixed foreign and native investment, such as Nuevo León, could have provided insights into Sonoran and Mexican economic development as a whole.

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JEFFREY D. NEEDELL. *A Tropical Belle Époque: Elite Culture and Society in Turn-of-the-Century Rio de Janeiro*. (Cambridge Latin American Studies, number 62.) New York: Cambridge University Press. 1987. Pp. xviii, 351. \$42.50.

This innovative reconstruction of Rio de Janeiro's Europhile social and cultural elite merits commendation on several grounds. Jeffrey D. Needell writes clearly and engagingly. He appreciates—even relishes—the brilliance of the *belle époque* without overlooking its derivative superficiality and its concealment of the benefits it afforded to the elites and their clients. His approach is refreshingly broad, treating high society as the product of national political events, economic change, the legacy of modernization and architectural innovation, and the impact of imported ideas and values—much in the spirit of the work of Richard M. Morse, the author's mentor, on São Paulo and of Carl E. Schorske on *fin-de-siècle* Vienna.

Needell argues that the elite's bold efforts to remake Rio de Janeiro on the model of Paris were a way to mask its shame at Brazil's "tropical lethargy" and bastardized national heritage. In the

roller-coaster world of boom and bust—a legacy of Brazil's neocolonial status—these borrowed values "suggested continuity and the legitimization that comes with tradition and identification with power" (p. 115).

The study explores the formal and domestic institutions of the elite and its socialization: the hallowed secondary schools (requiring empty memorization), the clubs, salons, theaters, and vacation habitats of high society, the literary pan-janira; family life, kinship, and matrimonial strategies. Needell argues that the elite embraced aristocratic Franco-English culture as a function of a larger process dependent on Brazil's "relationship to a world market revolving around the North Atlantic" (p. 153). He then applies Walter Benjamin's discussion of Marx's concept of commodity fetishism to the Brazilian case. For the Carioca elite, consumer luxury (from English cashmeres to French prostitutes) was knit to the elite's reality of domination and power in a fantasy world of a universal Franco-English culture. Nonelites—working men and women, rural migrants to the city, immigrant laborers, ex-slaves: all members of the faceless urban demimonde—did not fit into this closed and exclusionary vision of the modern metropolis.

The group portrait of the elite and its world raises questions for further inquiry. How did Elkim Hine, Antônio Januzzi, João Guilherme de Suckow (the Jockey Club's founder), and Humberto Gotuzzo get in? When did a Bahian planter like the Marquês de Abrantes or the *pernambucano* Sílvio Romero come to consider themselves and their families *cariocas*? What was the nature of the ties between members of the Rio-based national elite and their clans in the state oligarchies, traditional families that maintained solid political and economic control at least through the 1930s? We would also benefit from knowing more about elite attitudes toward the feared, seemingly invisible urban masses, shunned but ultimately coopted into the municipal political machines that keep the dominant stratum in power down to the present day.

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DARRELL E. LEVI. *The Prados of São Paulo Brazil: An Elite Family and Social Change, 1840–1930*. Athens: University of Georgia Press. 1987. Pp. 284. \$32.00.

This book represents a considerable reworking of the author's Yale University doctoral dissertation, published unrevised in 1977 by Editora Brasil-

ienese as *A família Prado*. Darrell E. Levi has enlarged his initial study by incorporating additional research drawn from published primary sources as well as secondary works, and he has substituted a forthright emphasis on dependency theory for most of the dissertation's stress on modernization theory, interpretively recasting a number of chapters in that light. Secondly, what might be called an Adlerian stress on psychohistory, while still detectable, has become muted and appropriately integrated within a more complex causal analysis. An entirely new introduction reviews research on family history in Brazil after 1970, placing the conclusions that literature offers within a comparative framework that includes specific references to the scholarly literature on European family history. One wishes that Levi's comparative reference had embraced the rest of Latin America, where recently a number of studies raise issues congenial to his concerns or relevant to his conclusions regarding the "rise and decline" of the Prados, one of Brazil's wealthiest and politically most influential families by the 1840s. This book, nevertheless, can be read as an extremely useful and enlightening volume offering comparative insights into a now impressively accumulating literature on the history of the elite family in nineteenth-century Spanish America.

Drawing extensively on the Prados' unpublished and sometimes previously unavailable private letters and diaries, as well as family reminiscences recounted to the author (together with a beautiful set of photographic portraits evocative of the strong personalities in the key generations, which has been reproduced in this edition), Levi creatively interweaves such primary sources with published genealogies, memoirs, essays, and political speeches in order to produce a careful reconstruction of six generations of the Prados. The arriviste Prados' early solidarity as a corporate family group rested on economic foundations carefully transferred in the 1850s and 1860s by Antônio da Silva Prado (baron of Iguape by 1848) and his much younger half-brother Martinho Prado from sugar and tax collecting to coffee plantations, railroads, and banks. Their parallel successes in entrepreneurship and patronage in separate urban and rural economic spheres proceeded under the undisputed dominance and extremely capable management of the patriarchal baron, the family architect who perpetuated his economic empire beyond his death in 1875. And he did so despite the emergence of considerable personal and ideological disagreement between, first, his presumably sole heir, Veridiana Prado, and her husband (and unilateral uncle), Martinho Prado, and then among their sons and grandsons. As a "modernizing" elite family, Levi argues, these

nuclear family Prados not only shaped the course of growth on São Paulo's western coffee frontier, by virtue of investment in the Paulista Railroad and by twice pioneering sponsorship of immigration (unsuccessfully in the 1860s and very successfully in the twilight of slavery), but also exercised a very special role, together with their mother, in opening up to European culture and salon life the cultural backwater that their provincial capital of São Paulo represented in the last decades of empire.

Some readers will object to Levi's designation of the Prados as a matriarchal family during Veridiana's years of legal separation from her husband and widowhood and will prefer to see those years at best as transitorily matrifocal. They may also prefer to ask to what extent Veridiana's inherited wealth and her family's position in national politics (not to mention her sociolegal position as her parents' presumably legitimized natural child) prepared her for rebelliously "divorcing" her estranged husband after over thirty years of marriage, suggesting social class as a powerful factor in her action and perhaps marking her as less iconoclastic than the author assumes. Others with an eye for kinship classification will take strong objection to Levi's idiosyncratic definitions for two non-nuclear Prado families (the extended family versus the *parentela*) according to a rigid family name formula that is not only disconcertingly logical (because it ignores historical practice) but also doubly gender-biased against Prado women. First, probably like the family genealogists he cites, Levi does not provide readers with the full (family) names of most of Veridiana and Martinho's daughters and granddaughters, although, one suspects, they may not have possessed such names in legal documents prior to the 1890 Civil Marriage Act. That they appropriated their husbands' family names is not at all clear. Second, the formula employed means that Prado men were ipso facto members of the extended family core, while most of their sisters moved from the nuclear family to the distanced *parentela* merely by virtue of their husbands' names. This logical determination goes beyond quibbling over names, for it distorts analysis. And the lack of a genealogical diagram with family names for the members of key generations and their spouses deprives the reader of the opportunity to assess the evidence related to collateral fragmentation and family recognition.

Levi's discussion remains, however, richly suggestive of the value that a study of one powerful elite extended family can offer for appreciating wider historical change. His illuminating comparison of the Prados' eventually limited entrepreneurial adaptabilities with those of the immigrant

industrial magnate Francisco Matarazzo and his offspring is just one convincing illustration of the appeal that this book exerts (although one wishes Levi had been able to extend the comparison to the Paraíba valley descendants of the first baron of Pati do Alferes, Francisco Peixoto de Lacerda Werneck, since Eduardo Silva's *Barões e escravidão: três gerações de fazendeiros e a crise escravista* [1984] would have permitted a livelier comparative foray into the classic dichotomy of "old" and "new" coffee zones and their dynasties). But, considering the limitless possibilities that an extremely talented (and numerous) family such as the Prados offers to historical researchers, Levi cannot be expected to be exhaustive. He has done well in raising and treating an impressive range of central issues confirming the historiographical importance of social history for rewriting yet another small part of a huge antiquarian literature that, happily, is now under aggressive intellectual siege in Brazil as an outdated iconographic tradition.

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JUNE E. HAHNER. *Poverty and Politics: The Urban Poor in Brazil, 1870–1920*. Albuquerque: University of New Mexico Press. 1986. Pp. xvi, 415. \$32.50.

This work considers what the abolition of slavery, the shift from monarchy to republic, mass immigration, and a rapidly rising tempo of urbanization and industrialization meant for the urban lower classes of Brazil. June E. Hahner begins her study in the 1870s, "when the imperial cities were at their peak," and ends in 1920, "when the incipient labor movement was severely repressed" (p. xiii). The rationale for commencing in the 1870s is not altogether clear to me, but the 1872 census does provide a convenient baseline for social change. The book focuses on the national capital, Rio de Janeiro, although the author frequently compares developments in São Paulo and elsewhere.

Themes include workers' organizations and political activity; strikes, riots, and other forms of protest; the problems of lower-class women in general and those of female workers in particular; the everyday lives of the poor at home and workplace; the special problems of immigrants; urban "development"; popular culture; and the elite's

repression and occasional cooptation of the urban poor.

Hahner devotes the larger part of her study to workers and their problems and points to their active resistance to various forms of oppression. She argues, however, that even anarcho-syndicalist leaders were conservative compared to their European prototypes in the period and that workers' frequent indifference to union activity may have been justified by the ineffectiveness of worker organizations. On the workers' indifference to unions, Hahner highlights union leaders' ambivalence toward women workers (often viewed as rate busters) and notes that males preferred the "protection" of female workers to equal pay (p. 233). Labor leaders were also ambivalent on whether women should be working outside the home at all.

In the politics of the period, there was little collaboration between middle and lower classes. The Jacobino movement of the 1890s, to which some of the urban poor were attracted, had no counterpart in the new century. The only other important antiestablishment movement before the 1920s, *salvacionismo*, made no appeal to the lower classes.

Hahner emphasizes the disruption of the lives of the poor in the great transformation of Rio de Janeiro under President Rodrigues Alves (1902–06). Nearly two thousand buildings were destroyed in beautification and urban renewal projects, and landlords tended to evict the poor, as property values of new and remaining structures rose (pp. 164–65). If the poor rioted or rebelled, they faced harsh reprisals. Such measures included the deportation of immigrants or transportation (of both nationals and foreigners) to the insalubrious Amazon, where laborers were required for rubber collection.

One might wish for more extensive treatment of certain subthemes, such as the rise and transformation of macumba and samba in this period, but, as the author warns the reader, the richness and depth of the social history of Brazil (for example, compared to that of France) is limited by the paucity of primary sources and monographs.

Hahner's thoroughly researched study is grounded in a wide array of archival sources and rare editions of worker and other popular newspapers. Methodologically it perhaps belongs more to the "old" social history, but the story is deftly told, and the work contributes signally to labor and urban history.

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Collected Essays

These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed.

CHARLES CHATFIELD and PETER VAN DEN DUNGEN, editors. *Peace Movements and Political Cultures*. Knoxville: University of Tennessee Press. 1988. Pp. xx, 317.

ROGER CHICKERING, War, Peace, and Social Mobilization in Imperial Germany: Patriotic Societies, the Peace Movement, and Socialist Labor. JOST DÜLFFER, Citizens and Diplomats: The Debate on the First Hague Conference (1899) in Germany. SOLOMON WANK, The Austrian Peace Movement and the Habsburg Ruling Elite, 1906–1914. WERNER SIMON, The International Peace Bureau, 1892–1917: Clerk, Mediator, or Guide? NADINE LUBELSKI-BERNARD, Freemasonry and Peace in Europe, 1867–1914. MARIA ANTONIETTA SARACINO, Woman, the Unwilling Victim of War: The Legacy of Olive Schreiner (1855–1920). JO VELLACOTT, Women, Peace, and Internationalism, 1914–1920: "Finding New Words and Creating New Methods." ELLY HERMON, The International Peace Education Movement, 1919–1939. NORMAN INGRAM, Romain Rolland, Interwar Pacifism and the Problem of Peace. KARL HOLL, German Pacifists in Exile, 1933–1940. DAVID S. PATTERSON, Citizen Peace Initiatives and American Political Culture, 1865–1920. HAROLD JOSEPHSON, The Search for Lasting Peace: Internationalism and American Foreign Policy, 1920–1950. CHARLES DEBENEDETTI, American Peace Activism, 1945–1985. RALPH SUMMY, The Australian Peace Council and the Anticommunist Milieu, 1949–1965. LAWRENCE S. WITTNER, The Transnational Movement against Nuclear Weapons, 1945–1986: A Preliminary Survey.

RAIMO VÄYRYNEN *et al.*, editors. *The Quest for Peace: Transcending Collective Violence and War among Societies, Cultures, and States*. (International Social Science Council Issue Group on Peace.) Newbury Park, Calif.: Sage. 1987. Pp. viii, 356. Cloth \$39.95, paper \$17.95.

JAVIER PEREZ DE CUELLAR, Foreword. DIETER SENGHAAS, Transcending Collective Violence, the Civilizing Process and the Peace Problem. ANTTI ESKOLA, Human Con-

sciousness and Violence. CAROL J. GREENHOUSE, Cultural Perspectives on War. KENNETH E. BOULDING, Peace and the Evolutionary Process. F. H. HINSLEY, Peace and War in Modern Times. RAIMO VÄYRYNEN, Global Power Dynamics and Collective Violence. A. CHUBARYAN and B. MARUSHKIN, The Inadmissibility of War in the Nuclear Age. ERNST B. HAAS, War, Interdependence, and Functionalism. JOST DELBRÜCK, Peace through Emerging International Law. YOSHIKAZU SAKAMOTO, Lessons from "Disarmament" Negotiations. ANATOL RAPOPORT, Conflict Escalation and Conflict Dynamics. KARL W. DEUTSCH, Peace, Violence, and War. NAZLI CHOUCRI and ROBERT C. NORTH, Roots of War: The Master Variables. CHRISTIAN SCHMIDT, An Economic Contribution to the Analysis of War and Peace. R. J. JOHNSTON *et al.*, The Geography of Violence and Premature Death: A World-Systems Approach. GIRI DESHINGKAR, Arms, Technology, Violence, and the Global Military Order. JOSE A. SILVA MICHELENA, Transforming the International Order: Options for Latin America. SOEDJATMOKO, Violence in the Third World. NIGEL YOUNG, Peace Movements in Industrial Societies: Genesis, Evolution, Impact. ELISE BOULDING, Learning Peace. JOHAN GALTUNG, Peace and the World as Inter-Civilizational Interaction.

Les Internationales et le problème de la guerre au XX^e siècle. (Collection de L'Ecole Française de Rome, number 95.) Rome: Ecole Française de Rome or Università di Milano. 1987. Pp. 390.

PIERRE MILZA, Introduction. JEAN-JACQUES BECKER, La II^e Internationale et la guerre. LILLY MARCOU, La III^e Internationale et le problème de la guerre: Bilan historiographique. PIERRE BROUE, Les Trotskystes et le problème de la guerre: Bilan historiographique. JEAN-PAUL JOUBERT, Le défaitisme révolutionnaire dans la stratégie marxistes. JOSE GOTOVITCH, Franc-maçonnerie, guerre et paix. ROGER AUBERT, L'Eglise catholique et le problème de la guerre: Bilan des travaux et état des problèmes. PHILLIPE LEVILLAIN, Le Saint Siège et la Première Guerre mondiale. RENE REMOND, Le Saint-Siège et la guerre pendant la Seconde Guerre mondiale. JEAN-MARIE MAYEUR, Les catholiques français et la paix du début du XX^e siècle à la veille de la Deuxième Guerre mondiale. PIERRE GUILLEN, Les Internationales et les crises coloniales avant 1914. CHARLES-ROBERT AGERON, Les forces internationales et la décolonisation de l'Afrique du Nord. SERGIO ROMANO, Le Internazionali ed i rapporti Est-Ovest

dopo la Seconda Guerra mondiale. ALFREDO CANAVERO, Pietro Nenni, i socialisti italiani e l'Internazionale socialista tra Est e Ovest dopo la Seconda Guerra mondiale. PIERRE MILZA, Les mouvements pacifistes et les guerres froides depuis 1947. BIANCA VALOTA CAVALLOTTI, L'Internazionale verde tra pace e guerra. GIORGIO RUMI, La Santa Sede e la scelta fra Est e Ovest (spunti da una rilettura dell'*Osservatore romano* 1947–1949). SERGE BERSTEIN, Le milieu genevois dans la France de l'entre-deux-guerres. OLE KARUP PEDERSEN, Small States and International Institutions. GEORG KREIS, Guerre défensive, sanctions militaires, guerre civile: Une controverse de la politique intérieure allemande (1926–1932). RENE REMOND, Conclusion générale.

MANFRED KOSSOK, editor. *Vergleichende Revolutionsgeschichte: Probleme der Theorie und Methode*. (Studie zur Revolutionsgeschichte.) Berlin, G.D.R.: Akademie. 1988. Pp. viii, 270.

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HARVEY J. KAYE, editor. *History, Class, and Nation-States: Selected Writings of V. G. Kiernan*. Cambridge, U.K.: Polity or Basil Blackwell, New York. 1988. Pp. x, 272. \$39.95.

V. G. KIERNAN: Seeing Things Historically. History. Gramsci and Marxism. State and Nation in Western Europe. Foreign Mercenaries and Absolute Monarchy. Nationalist Movements and Social Classes. Conscription and Society in Europe before the War of 1914–1918. Working Class and Nation in Nineteenth-Century Britain. Revolution.

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CARL LEVY, editor. *Socialism and the Intelligentsia, 1880–1914*. (History Workshop Series.) New York: Routledge and Kegan Paul. 1987. Pp. xiii, 301. Cloth \$59.95, paper \$19.95.

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Communications

A communication will be considered only if it relates to an article or review published in this journal; publication is solely at the editor's discretion. Letters may not exceed seven hundred words for reviews and one thousand words for articles. They should be submitted in duplicate, typed double-spaced with wide margins, and headed "To the Editor."

ARTICLES

TO THE EDITOR:

The discussion of intellectual history in the June issue is most welcome (*AHR*, 94 [June 1989]: 581–626). The questions raised are important, and the *AHR* is certainly the right place to raise them.

It is regrettable, however, that intellectual historians give so much attention to the epistemological coherence of history and so little to the actual impact of the theories and approaches of intellectual history on other fields. I for one am more dismayed at the growing isolation of historical fields than at the possibility that history is not ultimately a coherent order of knowledge, and it seems to me that intellectual historians should be concerned with this massive drift toward specialized irrelevance. Clearly, the contributors do feel this concern. It is all the more unfortunate, then, that they should spend so much time seeking to establish (or rediscover) "overarching framework[s]" (p. 599), a "cognitive element" (p. 620), or "communities of discourse" (p. 623). Is this not the very impulse that creates the problem of specialized isolation? Is it not precisely the striving to overarch that builds the little cocoons of standard images, questions, and debates in which most professional historians hide from the threat of new perspectives? Is all this yearning for positively defined teleologies and/or canonized traditions anything more than a continuing reassertion of the archaic prejudice that we are civilized and they are barbarians? Perspective is what intellectual historians should offer, and perspective

neither proceeds from nor requires a fixed framework. A venturesome historian of antiquity who argues (following Albert Olmstead) that the spread of Persian organization foundered on Greek barbarism begins his thinking with a vision, not a framework, and he may have much to contribute before anything resembling a framework emerges. Quite possibly, he never will come to one. Who cares? The same goes for a historian of Europe who rejects the concept of "age of discovery" and depicts the early modern centuries as the age of invasion. He could suggest interesting explanations as to why Sir Walter Raleigh's successors were more effective than those of Genghis Khan at conquest and genocide without having to drum up a new teleology to replace progress. Indeed, he might conclude his work with the thought that all such schemes constitute hindrances to the understanding of cultural history. He might even come to the realization that his main problem—the main problem of intellectual history—is to find ways to communicate a cultural perspective to clubs of entrenched specialists.

Specifically, the most pressing conceptual problem is to depict events and developments as cultural phenomena that are normally presented as links in teleological schemes or chapters in traditional lore. Intellectual historians are likely to be good at this. They have to be good at it. A remorselessly multi-cultural world demands approaches and methods that will drag self-styled experts away from culture-specific, judgmental notions dressed up as sciences and definitive histories. David Hollinger is to be commended for suggesting that intellectual history should be a commons open to all scholars (p. 610), but it is not enough for historians of culture to sit passively in their own back yard and welcome guests who happen to drop in. They are guardians of meaningful history, perhaps the only protection it has against the trivia of onward and upward (as in economic history), the banality of monument polishing (as in national histories), the hysteria of

decline and doom (as in Marxist history), and all the other teleological litanies that block efforts to perceive and explain an evolution in which the actors actually do something. It is not enough for our guardians to keep their museums looking orderly. The business of devising systems to distinguish Batman from King Lear should be left to librarians. The approaches and methods of intellectual history are needed *outside* the ever more restricted area that we are still allowed to call culture.

Michel Foucault was good at finding perspectives, and he was at his best when he invaded the preserves of specialized fields to point out new possibilities of interpretation. His occasional remarks about general principles were impressive only in the exceptional modesty he manifested when he made them. Frederic William Maitland is also a good role model. When he undertook to open up the historical study of English law, he kept his eye fixed on the law's evolution out of experience. Instead of worrying about schematic coherence, he puzzled out the relationship between his approach and that of the practicing lawyers. His main contribution was to introduce a new perspective into a very rigid field, and he did the job quite well. The awareness of perspective, he might have said, does not descend from an overarching framework but arises out of contests between differing perspectives.

Granted that it is not easy for an intellectual historian to step out of his own club and make his way among people who perceive new questions as untidy and new approaches as subversive. Despite the thunder of encouraging rhetoric that rolls unceasingly out of speeches by retiring presidents of the AHA, comparative and interdisciplinary work is not much encouraged. A new perspective throws an established field into disorder, and the first response is always a demand that contributions fit in with the established conceptual framework. It is more or less natural for would-be innovators to react by losing themselves in conventional arguments or else withdrawing to construct a new framework. Invaders are either smothered or excluded. Cultural phenomena are simply not allowed to appear in our citadels of teleology (for instance, in diplomatic, administrative, political, and church history) unless they are first explained away as pathology. Even the rare works of cultural history that defenders of citadels occasionally read and dutifully cite—Max Weber's *Protestant Ethic*, Friedrich Meinecke's *Machiavellism*, Carl Becker's *Heavenly City*, Johan Huizinga's *Homo Ludens*—have made very little impression. Still, it is a question of professional responsibility. If intellectual historians do not assert the authentic past

against the fossilizing hindsight of the schools, who will?

GEORGE YANEY

University of Maryland, College Park

TO THE EDITOR:

As the object of Joan Scott's belief that everything in the world is about herself, I reply to her *ad hominem* attack on my address to the November conference of the National Association of Scholars (AHR, 94 [June 1989]: 682). While noting her crudely ironic "sub-text" ("hysterical," "in a display of masculine bravery"), let me simply address her most defamatory remarks. She portrays me as an enemy of "democratic pluralism." She takes the sound-bite that has earned me my fifteen minutes of infamy ("the barbarians are in our midst") and informs the profession that "these 'barbarians' turn out to be those who advocate increased representation of women and minorities on faculties and in the curriculum." Finally, she offers me as an instance of the "self-designated guardians of orthodoxy" who see the "new" historians as "nothing less than a subversion of our national heritage and of the entire legacy of Western civilization." Revealing her rigorous empiricism and deep sensitivity to the study of regnant ideologies, she allows that "my source is, as cited, the *New York Times* report on the conference." (My source for what follows is a full tape of my every word before the NAS; anyone is welcome to stop by and hear it.)

My speech to the National Association of Scholars, in fact, was not about the "New History." It was not about *any* History. Indeed, it was not about any curricular issue. Participating in a panel on threats to academic freedom, I spoke about non-curricular threats to the freedoms of faculty and students. In a few asides, and in the discussion period that followed my panel, I referred briefly to what I indeed do believe to be the decline of the standards of evidence, logic, and intellectual coherence in the humanities and social sciences, although at no time did I cite "the New History" or any historian as exemplary of such tendencies. My examples, in fact, were from philosophy, literary studies, and musicology. If the shoe fits, however, Scott is welcome to wear it.

The comment about "barbarians in our midst," a rather commonplace symbol for the forces of unreason, referred specifically to two "small groups": those who would suppress the free speech of others and those unwilling to link their theories to inductive evidence. The second comment taken by Joan Scott from the *New York Times*, "Show them you are not afraid, they crumble" (a

conflation of two separate remarks), referred specifically to "bullies" who would deny basic academic freedom to others. Mild stuff!

Since Scott and the *AHR* have seen fit to bring my speech to the scholarly attention of my peers, let me correct the record and speak for myself. My address was about the partisan misuse of real problems of harassment as a pretext for curtailing free speech, for institutionalizing group rather than individual identities, and for empowering one particular ideological analysis of racial and gender problems and one particular set of self-appointed spokespersons for what in fact were diversified groups.

I said: "The anti-harassment movements on American campuses arise from a real problem: how to deal with illegal harassment. No student should be victimized or stigmatized on the basis, indeed, of race or gender. No student should be subject to sexual coercion or unwanted sexual attention in an academic setting." I reiterated these points and added "incidents of bigotry, fraternity brutishness and date-rape" to my list of real problems on campuses. What I objected to was "thought reform" in the guise of mandatory and highly ideological "racial and sexual awareness seminars" as sentences for or inoculation against harassment. I argued instead for "fixed behavioral penalties for behavioral infractions." My complaint was specific: "What has arisen to deal with the real problem of harassment are two things above all which have become vehicles for assaults upon academic freedom and intellectual tolerance," namely, the transference into the *classroom* of the workplace notion that no one should be "offended" by "intellectual behavior" regarding race and gender, and the privileging of *one* politicized view, without specifying that privilege, of who may take offense and why. My most essential criticism of new trends on campuses claimed that they were "based upon something that will not stand moral scrutiny: that we are on campuses of groups not of individuals, of group-minds, and of group-think and group-identities, rather than on campuses of critical *individual* minds." I objected, note well, to those who wanted "group identities rather than individuation [to] determine one's nature and one's place on a campus and in a society." I also objected to the view "that radical feminist theory is women's experience, that radical black analysis is the voice of a diversified American black community. Nothing of course could be more sexist nor more racist than the assumption that these congeries, 'female experience,' 'black experience,' can be reified into such programs."

Prominent libertarians have agreed with me. Commenting on the sorts of programs I described, Nat Hentoff, in the *Washington Post* (De-

cember 15, 1988), referred to "sensitivity training" at Penn as "sort of like a Vietnamese re-education camp." The legal column of the *Boston Phoenix* (July 28, 1989) described the new-age assault on freedom of speech on college campuses as Orwellian, compared it to Chinese universities today, and quoted from things that I have said and written on this subject over the past several years.

Scott may find my analysis evil or hysterical, but she will with difficulty transform it into "orthodox" opposition to democratic pluralism. In my panel, I expressed myself thus: "I think that one makes a mistake to argue as if cultural relativism were exogenous to the Western intellectual tradition. It seems to me that the choice is not whether universities should accept this or that orthodoxy with regard to the nature of value, but that the *debate* about value, which is at the heart of the Western intellectual tradition, go on unfettered . . . I attended the Princeton of mandatory chapel (cited as a paragon of moral education by one of the panelists). I did *not* find myself surrounded by saints or intellectuals . . . I much prefer my students of today, victims of the allegedly 'closed American mind,' to the students I knew in the early nineteen-sixties. *They* were indeed bigoted and anti-intellectual. Their education was an ongoing enterprise of learning social postures. I think it a great mistake for critics of the current assaults upon academic freedom and the current politicizations of American universities to idealize the past of American universities. I think it is a tarnished ideal. Indeed, one of the things I object to in the sorts of mandatory racial and sexual awareness seminars I denounced in my talk is *not* that they are the imposition of a *new* orthodoxy for an *old* orthodoxy, but that in some sense they are the imposition of an *orthodoxy* for issues that people ought to decide for themselves on the basis of a free, unfettered, classical education."

Authentic cultural conservatives understood full well their differences with me. In its editorial of November 1988 on the NAS conference, "Campus Report," the journal of *Accuracy in Academia* denounced my views in terms that Scott also would apply to me: "a sterile commitment to factual knowledge, operating in a moral vacuum." Unlike Joan Scott, however, they at least recognized the division in the NAS between what they termed "the value-neutral crowd" and "the traditionalists," and, unlike Joan Scott, they had the decency to offer my actual views before rejecting them: "Should they simply attempt to resurrect the academic ethic which demands that teaching and research be conducted without interference from the prevailing ideological winds? Or should they fight fire with fire, and promote a traditional set of values? In support of the first position, Alan

Kors argued: "The question isn't whether to adopt a particular value, but that the debate go on unfettered. Don't impose a new orthodoxy for an old orthodoxy." Many of the professors at the conference did not necessarily hold a conservative vision for the future of our society, with a corresponding set of policy prescriptions. They advocated a value-neutral approach: let us research in peace, and teach and write whatever truths we learn from our work."

Scott presumes to pronounce on my views of "the New History" and "affirmative action." Having been spoken for in these pages, I should be glad now to speak for myself. If by the "New History," Scott means "history from below," the empirical study of popular culture and ignored social groups, I favor it, admire much of it, and have learned extensively from it. Dare I say, without putting their reputations in jeopardy, that I read the work of Mona Ozouf and Sarah Maza, for example, with intellectual delight? I do think, however, that the historical profession often borrows methods for such study without adequate critical and skeptical examination, and that it tolerates increasingly tendentious scholarship that patronizes both audience and subject matter. I also believe that the current obsession with "culture" defined as "a system that grants hegemonic dominance" to one group at the expense of others pays inadequate attention to the countless ways in which culture is also an evolved system of symbiotic and survival-conferring interrelationships, changing as material conditions and developments in thought allow. Antonio Gramsci's notion of ideology has become a kind of philosopher's stone for too many historians, imbued with the alchemical power of transforming all data into therapeutic knowledge. I also believe that there is a great difference between the rigorous study of gender relationships over time and the latest phase of the war against testosterone. Historians who address the exceptionally difficult question of the relationship of the social and the biological might want to know a great deal more about the biological than they usually do, and they might want to explore the difference between biological determinism and notions of biological "preparedness."

If by "affirmative action" Scott means efforts to encourage individuals from all groups and classes to enter the academic disciplines, I favor it wholeheartedly. If by "affirmative action" she means the preferential hiring of individuals based on group identity, I oppose it wholeheartedly and believe it wicked. Professors who believe that they received their jobs as a result of discrimination against women and African-Americans should resign and reapply in open competitions: it is immoral of

them to ask innocent young whites and males to pay for their own guilty consciences. All individuals have the right to be judged on individual merit alone.

Since Scott has seen fit in these pages to quote from my letters to her, let me again set the record straight, since we are not old pals who exchange views from time to time. When I was told that she had presented me as an exemplar of bigoted critics of the "New History" in her address to a plenary session of the AHA convention, I wrote to her on January 9, 1989: "On the collegial (and humane) principle that one should confirm statements attributed to others, especially on sensitive matters, I write to ask if in fact (as reported to me) you described me as having referred to 'women and minorities' as barbarians at a session of the American Historical Association, and to ask if you would have the kindness to send me a copy of your remarks if such is available. I make no assumptions whatsoever about what you did or did not attribute to me, and would be most grateful to hear from you directly." I informed her that "I fortunately possess a full and unedited tape-recording of both my speech and my every comment before the National Association of Scholars," described the difficulty of getting accurate journalistic reporting of views such as mine ("people of my convictions tend to be lumped indiscriminately with cultural conservatives, bigots and others by journalists incapable of making often the most fundamental and obvious distinctions"), and assured her that I had spoken precisely *against* judgment of people by race or gender. Acknowledging her "reputation as a person of integrity who would want only honest debate," I wrote that "the people who were the object of my criticism are overwhelmingly white and male, two traits, however, that I consider accidental rather than essential to the issues of reasoned scholarship and the unabusive exercise of professorial authority." I concluded: "Looking forward to hearing from you, and profoundly apologetic for any waste of your time if the second-hand reports of your characterization of me were off the mark, I am, [etc.]"

Receiving no reply either to this letter or to phone messages, I wrote again to her on February 12: "I have tried repeatedly to contact you concerning your apparent declaration to the American Historical Association that I had pronounced 'women and minorities' to be 'barbarians' in my address to the National Association of Scholars. I did no such thing. As I indicated to you, there is a full tape-recording of my address and of all of my subsequent remarks at the convention. On January 9, 1989, I wrote to you at the Institute [for Advanced Studies]. On February 6, 1989, I tele-

phoned, leaving my home and office telephone numbers with your secretary, and asking if you would have the courtesy to return my call. Given the enormous damage done to my personal and professional reputation by your apparent remarks, surely I am owed a response to my request for clarification and for a copy of your speech. Once again, I ask if you could be so kind as to reply. I am eager to resolve this in a collegial, fair and decent manner."

Five weeks after that second letter, which is to say, almost three months *after* she used an article in the *New York Times* to assail my moral, intellectual, and professional integrity, never once seeking to clarify what I had or had not said, Scott at last replied, with no copy of her remarks, no explanation, and asking not for the unedited tape of my actual speech but for a "transcript" of my remarks. She may be a guru to many, but to me she seems a person without probity.

Finally, Scott, with delusional self-importance, claims that I see the kind of work she does as a threat to Western civilization. She may rest assured that I do not even see her as a threat to the Princeton corridor.

ALAN CHARLES KORS
University of Pennsylvania

Joan Scott does not wish to reply.

THE EDITOR

TO THE EDITOR:

In the debate on the "new history," John E. Toews censures "those for whom liberation is grounded on domination and citizenship entails pledge and exclusion" (*AHR*, 94 [June 1989]: 698). But this is precisely the grievance of conservatives insofar as the "new disciplines" are concerned. There is now a whole range of studies from which conservatives, including conservative historians, are virtually excluded from teaching positions in major U.S. universities. They are widely or wholly excluded because their views do not conform to those evangelical purposes that infuse these new disciplines, with their obligatory insistence on "sensitivity" and "compassionate concern"—their conviction that it is the scholar's role not merely to understand the world but to change it.

These new disciplines include women's studies, ethnic studies, Chicano studies, African studies—and, to some extent, religious studies. Imagine an Evangelical Christian teaching women's studies! The very thought seems bizarre. Discrimination takes many other forms. Conservatives are denied courteous treatment in public campus meetings

that deal with controversial topics raised by the new disciplines. Insofar as the new disciplines are concerned, conservatives are not invited to conferences or seminars. They are not adequately cited by their peers or even included in debates. Their work is widely excluded from assigned reading lists or even bibliographies. (I have tried to provide partial documentation as regards African studies in my article "African Studies: A Dissident's View," *Academic Questions*, 2 [Spring 1989]: 80–90.)

This issue cannot simply be swept under the rug. African studies, like other academic disciplines, needs public support. This does not mean that the political opinions of the professoriate in any given discipline should simply reflect the range of political opinions within the electorate as a whole. I do not ask for an Austrian-style proportionate representation system. But it is undesirable—to say the least—that a given discipline should become linked in the public mind with one particular set of orthodoxies. It is undesirable—to say the least—when a person such as myself has to advise conservative postgraduate students to stay out of an academic career in a particular set of disciplines. Conservatives form a considerable part of the taxpaying population. (Something like 30 percent of respondents in public opinion polls describe themselves as conservatives.) It is improper for them to be excluded a priori from appointments in designated disciplines.

To sum up, professional historians, like the practitioners of other social studies, should not confuse their role with the evangelist's. Joan Wallach Scott argues that "written history both reflects and creates relations of power. Its standards of inclusion and exclusion, measures of importance, and rules of evaluation are not objective criteria but politically produced conventions" (*AHR*, 94 [June 1989]: 681). This seems a dangerous doctrine. If she is right—if there are no "objective criteria," if all hinges on political power—her own discipline would rest on shaky foundations, vulnerable to any new shifts in the political balance of power. I trust, therefore, that she is mistaken.

L. H. GANN
*Hoover Institution on War,
Revolution and Peace*

Neither Joan Scott nor John Toews wishes to reply.

THE EDITOR

REVIEWS OF BOOKS

TO THE EDITOR:

In his otherwise favorable review of John Tate Lanning's book *The Royal Protomedicato: The Regulation of the Medical Profession in the Spanish Empire* (AHR, 91 [April 1986]: 496), Thomas Glick indicated that Lanning failed to point out that the Peruvian physician José Manuel Valdés was a mulatto. This simply is not so. Lanning introduces Valdés as a mulatto "born in Lima of a Spanish father and a free black woman" (p. 186) and repeatedly identified him as such in the chapter on blood purity.

MARK A. BURKHOLDER
University of Missouri, St. Louis

Thomas Glick did not wish to reply.

THE EDITOR

TO THE EDITOR:

A brief comment on Steven M. Stowe's review of Malcolm Bell, Jr., *Major Butler's Legacy: Five Generations of a Slaveholding Family* (AHR, 94 [February 1989]: 204-05) may be in order.

Stowe begins his review by noting that Major Butler created a large cotton and rice plantation on the Georgia Sea Islands at the end of the nineteenth century. Butler thereby established what Stowe calls a "somewhat ambiguous legacy." Later in the review, he repeats this characterization. "[I]t is not obvious," he writes, "just what Major Butler's legacy can be said to be." The ambiguity is of Stowe's creation, not Bell's. Bell makes it perfectly clear exactly what he means by "Major Butler's Legacy."

Bell deals with Butler's role as a member of the Constitutional Convention that met in Philadelphia in 1787. The major played a central part in fashioning the compromise legitimizing slavery in the new republic. Butler's legacy, Bell tells us, finds its "truest measure" in Frances Anne Kemble's *Journal of a Residence on a Georgian Plantation in 1838-9*, published in 1863. Kemble was the wife of Butler's grandson.

This legacy, as Bell defines it, was by no means a mere family, or private tradition, as Stowe would have it, but a bequest to Georgia, to the South, and to the nation itself. Stowe ought, perhaps, to have reminded his readers that Bell's book appeared on the occasion of the Constitutional bicentennial and was a contribution to the memorialization of that event. *Major Butler's Legacy* illuminates in human terms the nature of the "original intent" of the

1787 constitution builders and the tragic consequences that flowed from some of their most important decisions. That Butler, Bell writes, "who was certainly less than a champion of the rights of man, should have played a role . . . in fashioning the Constitution, is difficult to contemplate."

Stowe's blinkered approach to *Major Butler's Legacy* can hardly be accidental. It seems to mirror the "new" history's indifference to constitutional and political history as a basic aspect of the study of society. His review is a reminder of the continuing relevance for the historical profession of Gertrude Himmelfarb's article "History with the Politics Left Out" (*Harper's Magazine*, April 1984).

Since Alfred A. Knopf published the first modern edition of Kemble's *Journal* nearly thirty years ago, it has been widely used by historians of women's history and of African-American history, by teachers, archaeologists, and activists. Malcolm Bell, Jr., is to be thanked for helping to reintroduce Kemble's classic to the public and for pointing out its significance as part of the antislavery heritage of the American people.

JOHN ANTHONY SCOTT
Holland, Massachusetts

STEVEN M. STOWE REPLIES:

In my view, Malcolm Bell's fine saga is most importantly about neither the Constitution nor Frances Kemble's journal, the latter ably edited by John Anthony Scott. Bell subtitles his book "Five Generations of a Slaveholding Family," and I reviewed it as such.

STEVEN M. STOWE
Indiana University, Bloomington

TO THE EDITOR:

In his review of my book *Land und Freiheit: Machnovščina und Zapatismo als Beispiele agrarrevolutionärer Bewegungen*, Marc Raeff (AHR, 94 [April 1989]: 411) has accused me of being "mesmerized by irrelevant or artificial sociological models" in my comparison of these two agrarian revolutionary movements, and of uncritically resorting to Marxist vocabulary. It goes without saying that a reviewer has the right to dismiss the theoretical framework of the book under review as "irrelevant or artificial," and Raeff obviously has some objection to any attempt in historical research to make use of sociological models. But, in my view, any attempt in comparative research, if it is to be fruitful, should make use of a theoretical model—and, in contrast to Raeff, I am still of the opinion

that the use of sociological models might lead us to a better understanding of the historical past and is neither irrelevant nor artificial. I also do not accept that I resorted uncritically to Marxist vocabulary. Quite the contrary: I explicitly rejected Marx's and Lenin's view of the peasantry (pp. 28, 55, and 58). If, of course, the use of terms such as "mode of production," "agrarian capitalism," etc., is seen in this way, then I gladly admit to having used collocations similar to those of Marx. With regard to the "clumsiness" of my style, I ask potential readers to judge for themselves.

DITTMAR DAHLMANN

Heinrich-Heine-Universität Düsseldorf

Marc Raeff does not wish to reply.

THE EDITOR

TO THE EDITOR:

Normally, it is not a good idea to question the review of a book. Reviewers are entitled to their opinions of books, providing they are qualified to review and they follow customary review procedures for a scholarly journal. However, in the case of the review by Roger Fletcher of John C. G. Röhl, *Kaiser, Hof und Staat: Wilhelm II und die deutsche Politik* (AHR, 94 [April 1989]: 471–72), an exception should be made. There is no doubt that Fletcher possesses the scholarly credentials to review the book, but the manner in which he reviews it is hardly becoming to a journal of the quality of the AHR. It is more of a diatribe than a review.

In the first paragraph, Fletcher does an excellent job of establishing the place of the book under review in recent literature on the subject. Unfortunately, he goes astray in the second paragraph, which begins: "Methodologically, Röhl is an unrepentant 'Kehrite.'" One gets the impression that Fletcher uses the term as a historical slur against those who adhere to the tenets of Eckart Kehr, but John Röhl is not a "Kehrite." There is nothing in any of Röhl's writings to indicate that he subscribes to such views. In fact, Röhl has long argued against Kehr's thesis of the *Primat der Innenpolitik* (primacy of domestic politics). Whether one is a "Kehrite" or not is neither here nor there, but to attribute to an author views he does not hold and has even opposed is wrong and smacks of yellow journalism.

Fletcher demonstrates in his review that he is familiar with the various schools of historical thought disputing the position of Wilhelm II in Wilhelmine Germany. He claims that Röhl's thesis in the debate "is now more than unfashionable." A general reader of the review not familiar with

German historical controversies may get the impression that the case has already been settled, and Röhl has lost. Hardly. Within the next few years, a number of forthcoming biographies and books on Wilhelm II (including one by Röhl) will add considerable fuel to an already heated debate.

Rather than review the book that Röhl wrote, Fletcher goes on at length about subjects that were not covered or that received only scant mention. For example, Fletcher writes: "Much emphasis is attached to the minimal brain damage possibly inflicted on William at birth, causing a condition now known as attention deficit disorder, or ADD." In fact, Röhl makes only one cautious, guarded reference to the possibility of brain damage on page 33. He does not refer to ADD at all. Perhaps he will deal with this subject in his biography of Wilhelm II, but he does not do so in the book under review. A single guarded comment hardly constitutes "much emphasis."

Internally, Fletcher's review lacks a sense of cohesion and direction. In the second paragraph, he states that Röhl based "his case on meticulous and extensive archival research, as well as a thorough grasp of the prodigious secondary literature in several languages." However, his final statement in the review seems to say that Röhl's forthcoming biography of Wilhelm II should be more scholarly than the book under review, which Fletcher claims was intended to satisfy the "demands of the nostalgia boom in the Federal Republic." I would agree with the first statement regarding the "meticulous and extensive archival research" but not with the idea that the book satisfies the nostalgia boom in Germany. While the book contains much interesting information on Wilhelm II and Imperial Germany, nostalgia buffs are hardly going to warm to some of the ideas presented, including, by Fletcher's own admission, the section on the "war council of December 8, 1912."

Fletcher has a flair for writing, an overall grasp of the subject, and opinions that add to the debate, as evidenced by his introduction to and translation of Fritz Fischer, *Bündnis der Eliten (From Kaiserreich to Third Reich: Elements of Continuity in German History 1871–1945)* (1986). However, a reviewer must review the book at hand, not what the author may present in some future book; nor should a reviewer be allowed to use the columns of the AHR to present his or her views under the guise of a review. Historical disputation should not be stifled; far from it. Disagreement infuses more life into our profession. However, it must be done in a scholarly, professional, responsible manner and in the proper forum.

ANDREW R. CARLSON
Kalamazoo, Michigan

Roger Fletcher does not wish to reply.

THE EDITOR

TO THE EDITOR:

It is contradictory and misleading for Peter Jelavich to end his discussion of *History, Politics, and the Novel* (AHR, 94 [June 1989]: 702–03) by urging me to develop “a more inclusive rather than exclusive approach to new developments” after a review in which he proceeds dogmatically to question “the degree to which [I am] engaged in historical research” at all. One should indeed be both weary and wary of attempts to dismiss as nonhistorical approaches to history with which one disagrees on grounds one does not deign to explicate and defend.

Jelavich confesses that “it is hard to purge a residuum of the old *wie es eigentlich gewesen* standpoint from one’s blood.” Yet he attributes to me a view I do not express—a view I have in fact criticized. He writes that, for me, novels are “possibly political—insofar as they liberate the mind for a ‘free play’ of thought and aspiration.” In marked contrast to “free play,” I insist upon the variable interaction between work and play in literature and in life.

Jelavich implicitly identifies history with a rather narrow contextualizing historicism. I criticize both historicism and presentism as false options and defend an approach that stresses an exchange between past and present with import for the future. Elsewhere, I have tried to develop more extensively the theoretical basis of this approach through the argument that we are implicated in transferential relations to the past that we should neither deny nor simply indulge in (or act out) but instead critically and dialogically work through. Relying on stereotypical oppositions, Jelavich identifies this approach with the presentism I criticize, and he proceeds to include formalism in his abstract if not formalistic categorization of my approach. But, just as a concern with the relationship between past, present, and future should not be equated with presentism, so an interest in form is not tantamount to formalism. Jelavich fails to notice that I treat matters of form (such as narrative structure, voice, irony, and parody) in a non-formalistic way precisely because I explore the question of their ideological and political role.

The most distinctive feature of Jelavich’s review is that he contents himself with a couple of quotations culled from the introduction and from the conclusion of my book and says virtually nothing about its excluded middle: the specific discussions of a series of important novels. Here, the limitations of a certain approach to history become

blatant, and they cannot be blamed entirely upon constraints imposed by the format for a relatively short review. Jelavich simply does not seem very interested in whether specific interpretations open thought-provoking dimensions of texts or help to engender significant critical insights in the reader. His primary concern seems to be whether an approach conforms to a predefined conception of proper history. I doubt whether the emergence of a “stunning hybrid” can possibly be furthered by this preoccupation with boundary maintenance.

Jelavich apparently would have been overjoyed had I found a few *bras nus* who proclaimed that they became revolutionaries because of the way they read *Le rouge et le noir* or had I relied on a letter from Stendhal in which he said he did or did not intend a result of this sort. I do not deny the value of this kind of research and even acknowledge that, in certain ways, it may be complementary to what I attempt to do. But it was clearly not essential to my project in this book, and it does not provide an inclusive and exhaustive definition of historiography. Jelavich’s conviction that he already knows what counts as real history serves to release him from the obligation of actually reviewing my book and enables him to obscure its central problem: the manner in which texts come to terms with significant contexts in complex and diverse ways. His “review” comes very close to being an act of pure disciplinary politics in its unargued, dogmatic assumption that a deeply imprinted, dubiously procrustean, perhaps dominant, yet increasingly contested conception of history is history *tout court*.

I do not believe that thought progresses through the premature attempt to do everything at the same time. I think it moves forward through pointed initiatives that reveal the deficiencies of hypertrophic growths (including overcontextualization) and goes further in directions currently underdeveloped (such as close, critical reading in historiography or historical inquiry in literary criticism). In my book, I focus on basic socio-cultural, political, and literary contexts, and I stress that a crucial issue is how we select and weight contexts and how we analyze their specific relations to different kinds of texts. I certainly think scholars in humanistic and social-scientific disciplines should work out better articulations between the understanding of contexts and the interpretation of texts. But this project cannot be advanced by simply proffering more and more information about authorial intentions and the immediate *Umwelt* of texts. A more useful critique of my book would have indicated how specific contextual considerations I allegedly ignore might in fact have led to better, more genuinely informed and provocative readings—readings that would not pro-

vide a rounded-out historicist tableau or furnish an unearned and deceptively satisfying sense of closure but would actually promote critical and self-critical exchange between past and present. I would invite such a critique because it would indicate directions in which we might go in elaborating a fruitfully hybridized, cross-disciplinary mode of analysis.

DOMINICK LACAPRA
Cornell University

PETER JELAVICH REPLIES:

I am somewhat surprised that Dominick LaCapra accuses me of "stereotypical oppositions," because the polarities I employ in my review are those he puts forward himself. He uses the dichotomy of "overcontextualization" and "formalistic hypostatization" to describe the current state of literary studies, and he claims that he will transcend it. My contention is that he has not succeeded in doing that, and he ends up instead close to the "formalistic" camp.

Of course, he is correct to say that I would have to speak more of the "excluded middle" to validate my point. I do in fact plead the "constraints" of a "relatively short review" as mitigating circumstances and thus am pleased to have this opportunity to go into somewhat more detail. Let us look at his sections on Thomas Mann, which account for a quarter of the "excluded middle." Aside from a reference to Mann's well-known anxiety over his ability to attain "greatness" (p. 119), the "biographical context" for *Death in Venice* is limited to half a page that makes the rather obvious point that the protagonist, Gustav Aschenbach, coincides with some aspects of Mann's life but contradicts others (p. 116). Although LaCapra proceeds to contend that "the general historical context is also internalized in a complex way," the ensuing pages do not elaborate the point with any specificity. What we receive instead is a discussion of the internal workings of the text—one that is certainly adequate but not exceptional, given the sophistication of the critical literature on Mann. Indeed, the "critical context" is limited to a short discussion of Erich Heller and a passing reference to Georg Lukacs. The chapter on *Doktor Faustus* does not deal with any critics at all, and it hardly refers to the "historical context" beyond information that can be gleaned from the novel itself. (In particular, it is surprising that LaCapra did not ground his ruminations about Jewish figures and stereotypes in the novel by examining this issue in terms of Mann's life and other works, about which there has been considerable debate.) LaCapra may be

satisfied that he has "focused on basic socio-cultural, political, and literary contexts," but I am not.

LaCapra asks how "contextual considerations" that he "allegedly" ignores would lead to more "provocative" readings. For just one example, let's turn to Mann again and to one of LaCapra's favorite themes. LaCapra stresses the complex and problematic workings of irony and parody in Mann's work (hardly a new discovery), but if we look beyond the text to the author, we find the issue even more "provocative." Mann endorsed the Bavarian state's imprisonment of the satirical and "blasphemous" author Oskar Panizza. Furthermore, Mann's response to the furious Lex Heinze debate—a piece of legislation that would have curtailed much "modern" artistic expression—was to write "Gladus Dei," a work that ironically "stood above" the fray by parodying both sides. Had all artists reacted that way, the legislation would have passed; instead, thousands protested, and the bill was shamed into defeat. What does this say about Mann's supposedly "regenerative" use of satire, parody, and irony? It says that Mann was well aware that some forms of satire and irony could provoke repression (Panizza), while other types had the potential to collude with repression (his own "Gladus Dei"). It also says that, on two significant occasions, when the public life of culture was at stake, Mann sided with Jesse Helms against Andres Serrano and Robert Mapplethorpe, so to speak. Mann's response suggests that his attitudes toward irony, parody, and satire were even more complex and ambiguous than LaCapra indicates. I would argue that much of that complexity resided in the tension between Mann's conflicting views on irony and satire "inside" and "outside" his own texts. Moreover, his attempts to grapple with that tension gave a dynamic to his literary development: having changed his political tune after 1918, he explicitly questioned the validity of parody by the time of *Doktor Faustus*—all the while continuing to use ironic and parodistic devices. In short: whereas LaCapra claims that "overcontextualization" diminishes a text, I would contend that contextual detail can elucidate further the "provocative" complexity of literature.

Because I do not believe that LaCapra has accomplished the goal he sets out to achieve, he says that I "[do] not seem very interested in whether specific interpretations open thought-provoking dimensions of texts or help engender significant critical insights in the reader." LaCapra's self-congratulatory sentence implies that he considers himself the arbiter of what is "thought-provoking" and "significant." Be that as it may, I never questioned the significance of LaCapra's queries; I questioned his ability to transcend the

bounds of internal textual workings and thus to engage in "historical arguments." Because of that, LaCapra claims that I have committed an "act of pure disciplinary politics." That charge is simply silly (not to mention a trivialization of Foucault). My review even compliments him on his contributions to historiography, his ability (elsewhere) to underscore problems in reading and writing about history. In fact, my criticisms of this particular book are ones that LaCapra himself has leveled against other scholars on other occasions. If that makes me a "dogmatic" disciplinarian, what does it make him? LaCapra, who faults so many other scholars, seems to want to occupy a space where he is free to criticize others yet be exempt from criticism himself. At the very least, that is a misapprehension of the "context" of the historical profession.

PETER JELAVICH
University of Texas, Austin

TO THE EDITOR:

In his review of Kevin Christiano's *Religious Diversity and Social Change: American Cities, 1890–1906* (*AHR*, 94 [June 1989]: 859), Maris Vinovskis suggests that Christiano might not fully appreciate the limitation of the data he uses: "further inquiry into the quality of those data is warranted. Indeed, he (Christiano) acknowledges that Gregory Singleton, using the same data for Los Angeles but supplementing them with information from parish and denominational records, arrived at a quite different index of religious diversity." I have no desire to quibble with Vinovskis's intelligent and balanced review, but I would like to offer some clarification.

Early in the preparation of his study, Christiano contacted me to discuss significantly different values for the index of diversity values he had derived and those included in my *Religion in the City of Angels: American Protestant Culture and Urbanization, Los Angeles 1850–1930*. My supplementary procedure was ambiguously explained in a methodological appendix (shortened by editorial fiat from the original manuscript). Christiano noted the difference and explained the discrepancy in a footnote, for which I am grateful.

For the study of any given locality, the use of supplementary material is obviously preferable. For the sort of comparative study Christiano undertook, however, such augmentation would be logistically problematic and a methodological nightmare. I believe Christiano was absolutely correct to use only the standardized printed material for his comparative study. Furthermore, while the values in my study are therefore dif-

ferent, and while the pattern for Los Angeles is somewhat of an anomaly, our studies are conformable, and the differences are the result of separate conceptualizations, data bases, and methodologies for significantly different tasks. Christiano is a careful and conscientious scholar, and I concur wholeheartedly with Vinovskis's final assessment that "the book is well worth reading because it opens up new areas for scholars of religion to explore and challenges us to utilize hitherto neglected religious census data" (p. 860).

GREGORY HOLMES SINGLETON
Northeastern Illinois University

Maris Vinovskis does not wish to reply.

THE EDITOR

TO THE EDITOR:

I am sorry to see historians lending credence to the idea that President John F. Kennedy recruited the Mafia in the CIA's effort to murder Fidel Castro—at least, that is how one must interpret the murky reference in Robert Freeman Smith's review of Trumbull Higgins's *The Perfect Failure* (*AHR*, 94 [June 1989]: 897) to "Kennedy's involvement with the Mafia in Castro assassination plots" and his dark allusions to "probably more unrevealed aspects of a Mafia connection."

If Smith will refresh his memory of the Church Committee investigations, he will discover that the CIA made its approach to the Mafia during the Eisenhower administration, *not* during the Kennedy administration, and that the CIA had already installed Sam Giancana and John Rosselli in the Kennilworth Hotel in Miami by September 1960, weeks before Kennedy's election and months before his inauguration. In short, the employment of the Mafia was an initiative of the Eisenhower administration, not of the Kennedy administration.

If Smith is suggesting that Kennedy's involvement with Sam Giancana's friend Judith Campbell Exner exposed him to Mafia blackmail, he forgets that Robert Kennedy's Justice Department sought in 1962 to prosecute Giancana for having placed an illegal wiretap in 1960. (In fact, the tap had been placed by the CIA to protect Giancana's amorous interests in Las Vegas while he was off plotting Castro's assassination in Miami.) Neither the CIA nor Judith Exner could save Giancana from Robert Kennedy's determination to send him to prison, where indeed he finally went for the first time since 1942. And, if President Kennedy was subject to Mafia blackmail, how does Smith explain the current view that the Mafia so feared

and hated the Kennedys that they organized his assassination?

ARTHUR SCHLESINGER, JR.
*Graduate School and University Center
 of the City University of New York*

ROBERT FREEMAN SMITH REPLIES:

I fully realize the desire of the veterans of Camelot to try to protect the sacred flame that is the myth of the Clan Kennedy. And I am somewhat flattered to be included with Christopher Hitchens in the ranks of those who must be chastized for suggesting that there existed some kind of link between the Mafia and the brothers Kennedy. Interested readers, by the way, may want to read the Schlesinger-Hitchens exchanges that have appeared in the letters section of the *Times Literary Supplement* since late in 1988. Yet I think that the professor doth protest a bit too much.

Arguing over this issue is a lot like mud wrestling—it can get messy, and the decisions are rarely clear-cut. Much of the evidence is circumstantial and the major participants have been murdered. I seriously doubt that any written assassination order will ever be found. After all, Henry I did not produce any memos saying, "Eliminate Thomas Beckett." Still, the circumstantial evidence is strong enough to warrant my short statement in the book review, and I would refer the interested reader to the following books: Warren Hinckle and William W. Turner, *The Fish Is Red: The Story of the Secret War against Castro* (1981) (see especially p. 107), Peter Collier and David Horowitz, *The Kennedys: An American Drama* (1984), and C. David Heymann, *A Woman Named Jackie* (1989). Of course, Schlesinger dismisses as "unreliable" any person or book that disagrees with his views, so an exchange would only get us into intellectual mud wrestling.

Interested readers should be aware that one piece of direct evidence surfaced in February 1988, and I referred to it in my review. Judith Campbell Exner, who is dying of cancer and no longer fears assassination, revealed that she served as a courier between JFK, Sam Giancana, and Johnny Rosselli for about eighteen months in 1960 and 1961. She reports that she regularly carried sealed envelopes back and forth between Kennedy and the Mafia leaders. She did not read the contents and does not claim to know what information she carried. From conversations, she surmises that the 1960 communications dealt with the West Virginia primary and the general election. She also stated that she arranged meetings

between Giancana and JFK. We will probably never know for sure what transpired between the two. Perhaps, after the election, the president only wanted gambling tips or advice on investments in Las Vegas casinos. But a link did exist. Fidel Castro was very much on the president's mind, and the Mafia was involved in CIA assassination plots. (Yes, the mob was recruited prior to the Kennedy election.)

Schlesinger's final gambit is to ask rhetorically how I explain the "current view that the Mafia so feared and hated the Kennedys that they organized his assassination." Indeed, several recent books do make a serious case for Mafia involvement in the president's assassination, and the evidence is entirely circumstantial. Yet, if the Mafia did kill Kennedy, it would only strengthen the case for a prior relationship. Organized crime bosses do not, as a matter of course, assassinate prosecutors. In the case of the Kennedy brothers, however, the Mafia believed it had been *used* and then *betrayed*. This scenario called for revenge, and the assassination of the president himself becomes more plausible (and logical within the framework of mob mentality).

I think that what Schlesinger really fears is that John F. Kennedy will replace Warren Harding as the butt of classroom ribald stories. Indeed, the more we find out about the libidinous JFK, the more innocent Warren Gamaliel appears.

Schlesinger has a perfect right to interpret the murky evidence any way he wants. He should concede that right to others. As Charles A. Beard put it, "We hold a damn dim candle over a damn dark abyss."

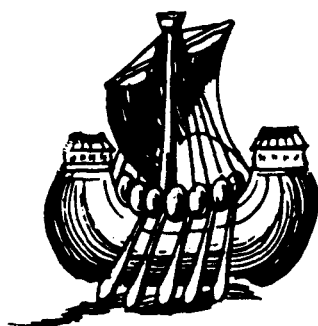
ROBERT FREEMAN SMITH
University of Toledo

ERRATA

Konrad H. Jarausch served as the chair of the opening session of the Annual Meeting of the American Historical Association in Cincinnati, December 1988, and not James H. Billington, as reported in the June 1989 issue of the *AHR*, p. 654.

There was a typographical error in the article by John E. Toews, "Perspectives on 'The Old History and the New': A Comment" (*AHR*, 94 [June 1989]: 698). In the phrase "those for whom liberation is grounded on domination and citizenship entails pledge and exclusion," the word "pledge" should have been "privilege." We regret any misunderstanding this has caused our readers.

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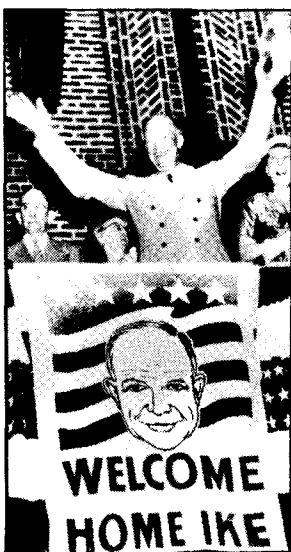
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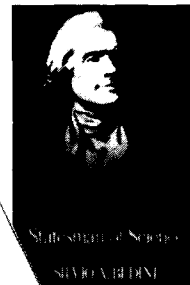
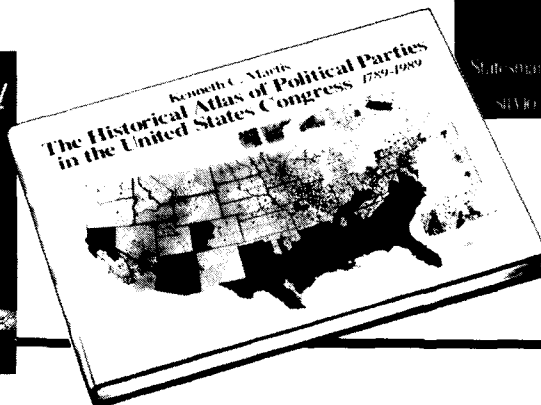
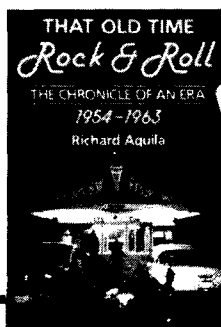
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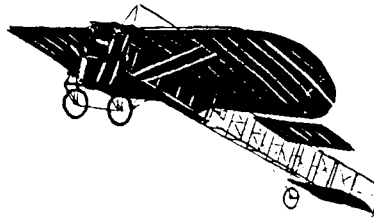


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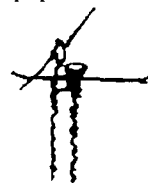
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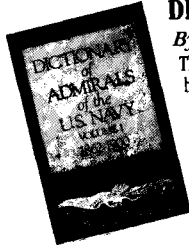
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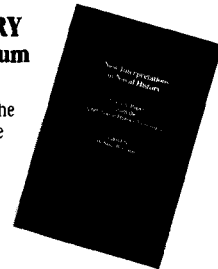
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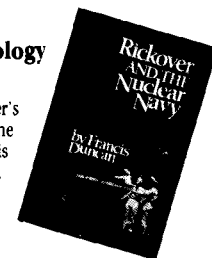
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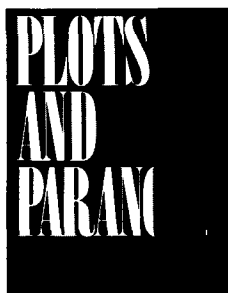
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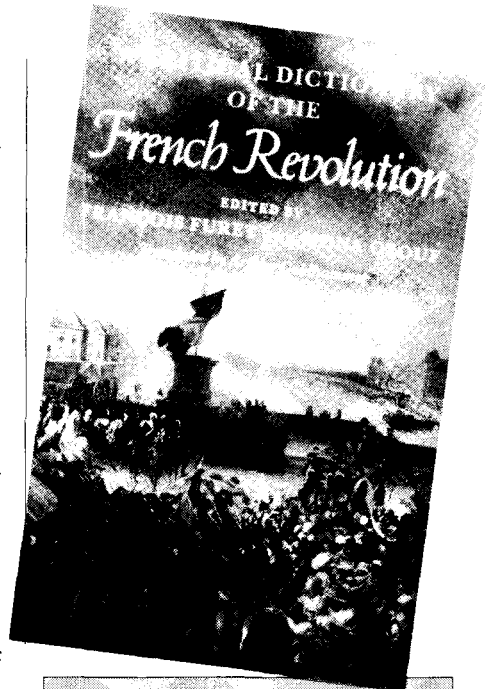
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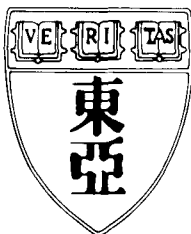
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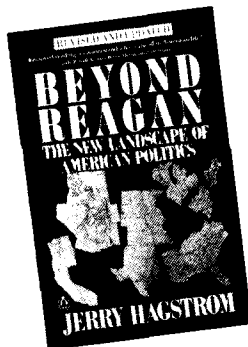
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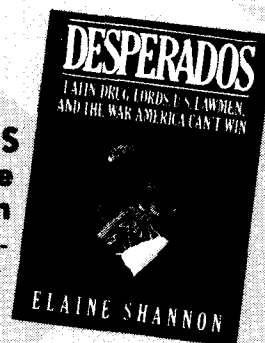
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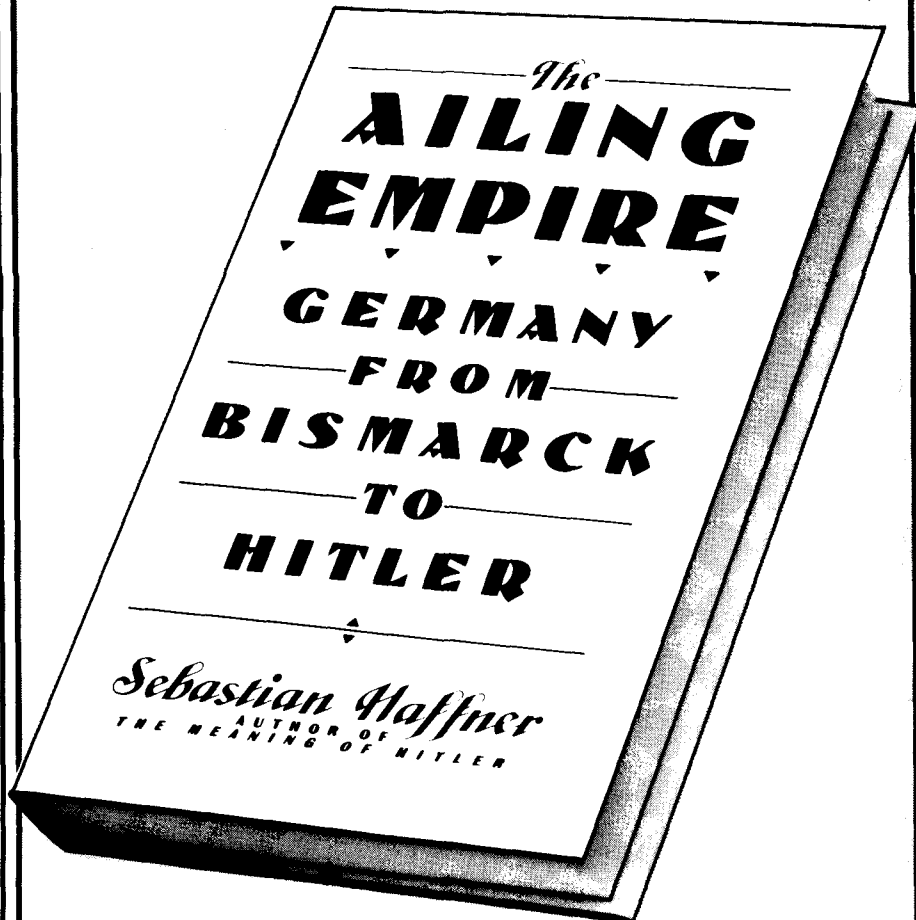
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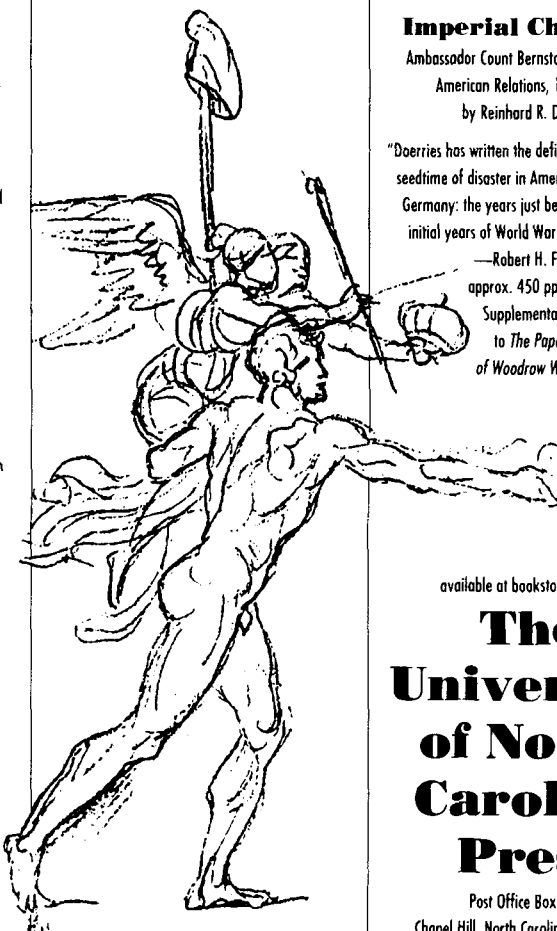
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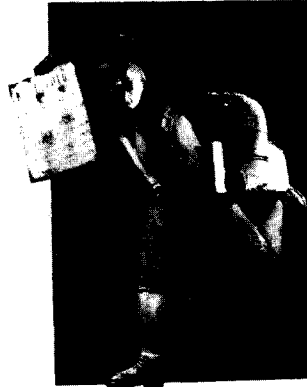


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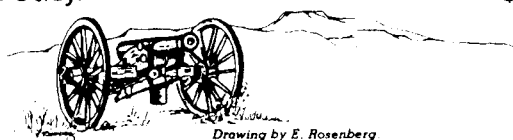
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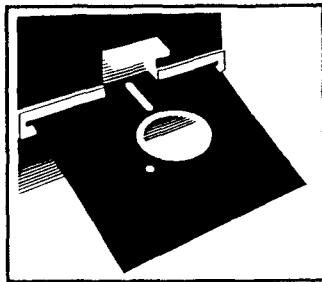
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Part of the Culture and Comfort exhibit, which shows the history of upholstery in the United States, is on display in the Exhibit Room beginning on Thursday, December 31.

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